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THE PSYCHOLOGICAL BULLETIN

GENERAL REVIEWS AND SUMMARIES

PSYCHOLOGICAL PROGRESS IN 1910

BY PROFESSOR EDWARD FRANKLIN BUCHNER

Johns Hopkins University

The presumption of psychological progress implies a recognizable independence of psychology as a science, either as to subject-matter or as to method. At least the purposes of historic record require some appreciable achievement in a field not appropriated by another discipline. It would seem, therefore, that there would be some working agreement upon the fundamental features of consciousness. The year just gone has shown quite the opposite picture. Some of us are still struggling at initial clearness as to what psychology is about.

Pikler gives a new expression to his views of the place of consciousness in the order of material phenomena (27). Without the advantages of the physical sciences, which lead us to resolve everything into outside physical forces, we could have no clue as to its nature and function. Neural opposition is the sole occasion for consciousness whose function is merely the production, in the most economical way, of adjustment between the organism and the external world. A dynamic psychology is thus possible because the analysis of physical facts becomes the analysis of mental facts. Yerkes' solicitation of opinion from a selected group of American biologists brought to light 'the sad truth that today psychology means very different things even to psychologists themselves' (40). "As compared with even the newest of the physical sciences, it appears crude, vague, inexact, unscientific," possibly because it "is a dismal mixture of physiology and psychology." Champions for consciousness as the creditable material for scientific treatment are, of course, not wanting. For example, Bolton (5) argues for the special efficacy of consciousness on the ground that it alone can meet the need of those individual

adjustments which are needed more quickly than they can be supplied by the more slowly acting instincts of the organism. And Judd reverses the whole setting of these uncertainties by arguing that "the concept of consciousness is most productively utilized in science when it is treated as a cause," inasmuch as by its present organization it determines the mode of future action (17).

Despite these possible opposite views of psychology, the work of gathering new material and furthering the interests of the science as an independent subject continues. In view of the leading tendency noted in our last survey, it is not surprising to meet with such a book as McCabe's (22). It presents the results of a synthesis of widely outstanding results from all the sciences into a view which shall let us see how, as far as possible, the complete development of consciousness has taken place. He undertakes 'the cosmic problem of mind' as it appears in terrestial evolution. Can mind be brought within the reconstruction of the whole process of development? For answer the author ventures an examination of the recent work of geology, paleontology, biology and neurology. The work "does not purport to be a contribution to psychology, since psychology disavows interest in such an issue." Mind is looked upon as one of the culminations of the evolutionary series. It is a bold attempt to recover the traces left in its natural history. Consciousness seems to have made its appearance in the Tertiary period, beginning in the mammals and rapidly developing in the primates. While McCabe takes mind as literally present in the evolving series, Crawley (11) raises the question as to how the race has come into possession of its idea of the soul. The one book examines primitive human culture, the other the natural antecedents of all human activity. The answer to the question, Whence comes the idea of the soul? becomes indeed a kind of psychology of psychology. The now well-known theories that the idea of the soul had an animistic origin, or came from abnormal states, trances, or visions, or dreams, are rejected. They are replaced by a novel suggestion which turns to certain mental processes in the lowest stages of evolution. The hypothesis calls for the origin of the idea in the memory image, chiefly visual, as dissociated from the percept. Anthropology is made to supply the data used in the analysis and the argument.

In recent surveys we have had occasion to note the steady rise in the appreciation of the practical values of psychology. This phase has advanced so far that it must be said unhesitatingly to be the leading feature in the period under review. In his choice account of

the recent results in experimental psychology in the Clark University celebration address, Titchener (35) voices anew his belief in 'pure science' and declares against the possibility of any application of its results. "So far is experimental psychology from any general readiness to furnish ideas for application, that applied psychology has been obliged to think out ideas for itself; and so far is applied psychology from reliance upon the parent discipline, that some of its most widely used and most strongly emphasized ideas contravene established scientific principles." On the other hand, Binet (3), for example, recently announced in the *L'Année psychologique* the purpose of limiting and defining the field of this useful annual survey to those interests in psychology which are particularly practical, such as the measure of intelligence in children, and the problems which arise in pedagogy, law, pathology, and industrial organization. In an age saturated with the development of a pragmatic theory of life and reality as its final philosophy, it becomes a matter of serious doubt whether the one field of science which has been father to the movement can possibly stem this tide towards the practical and continue undisturbed in its devotion to its ideals of a pure science.

The most conspicuous contribution of the year, in the light of the foregoing drift in psychological affairs, is the possession of a working program of 'tests' which Whipple (39) has offered. During a period of nearly two decades the refinement of the experimental methods has consciously urged psychologists, individually and collectively, to seek some acceptable standards which should be true to the science and available as measurements of a given individual. Following the stream of suggestions, this manual makes a new move in the direction of elaborating 'a scientific system of mental tests.' In giving the history, the technique, and some of the results obtained by the application of each of the fifty-four tests selected, this book becomes an exceptionally advantageous survey of the entire field of this type of scientific work. The spirit of this undertaking is properly tentative. But its results ought to do much towards securing a standardization of tests, and advancing the interests of experimental pedagogy. From the material gathered by applying twelve tests upon the reaction, memory, apperception and attention processes of two groups of school boys, Burt (7) undertakes to answer definitely the questions of general intelligence. "Parental intelligence may be inherited, individual intelligence measured, and general intelligence analyzed; and they may be analyzed, measured and inherited to a degree which few psychologists have hitherto legitimately ventured to maintain."

In view of the limited space allotted to our retrospect in the reorganization of the BULLETIN, we must confine ourselves to mentioning some of the changes in the literature due to new works, revisions, and special summaries, and to noting a few contributions deserving of special mention. The completion of the sixth edition of Wundt's *Grundzüge der physiologischen Psychologie*, our greatest foreign classic, is a striking tribute to the change and expansion in the material now available in the science. A similar advance in experimental aspects is further attested by the appearance of the second part of Titchener's text-book (**36**). It is not a revision but a new book which Calkins (**8**) has given in her latest effort to expound 'self-psychology.' 'Idea' psychology is more or less eliminated, and experimental material is noticeably absent, while 'what appear to be the important results of so-called functional psychology' are incorporated. The pedagogical problems of the subject seem to share large responsibility for these last two books, while the growing importance of educational psychology is to be accredited with several other revisions. The appearance of the fifth edition of Sully's work (**33**) nearly a quarter of a century after its first writing (1886), although modified, gives one a good opportunity to see how much psychology has changed within the interval. In its old-time retention of introspection, its re-affirmation of the importance of mental analysis into the tripartite classification, and its refusal to accept the suggestivenesss of the sensori-motor arc, this fifth edition would seem to come almost as a protest against progress. An instructive contrast appears when one compares Thorndike's contributions (**34**) now at hand in an important revision. In this field which he has made so largely his own, the methods of exact science are given application in approaching the problem of the natures of individual men. Münsterberg (**24**) has added his volume which seeks to trace certain closer connections between psychology and education. In the enlarged third edition of Eisler's dictionary (**14**), based on historical sources, the terms of psychology are given ample space.

The wide extension of the investigated fields of psychology and the rapid increase in material contributed by researches have long since made the résumé or summary of a restricted topic a useful contribution to the literature. Not infrequently a survey is made the basis for one's own ideas. Among those recently appearing mention might be made of the following. Arnold (**2**) has presented a novel accomplishment in his interesting effort to arrange and interpret the conclusions of various experimental investigations. An extended

résumé of modern work on memory is presented by Piéron (26), which begins with the permanent tendencies in inorganic matter and ends with memory in man. While it may be misleading to have us think of the physiological process in plants as 'memory,' it is helpful to have the suggestion that memory is a social, as well as a biological phenomenon. The problems of feeling have been restated by Ribot (30), in light of the general position that holds affective consciousness to be both analytically and genetically different from cognitive consciousness. In the posthumous work of Vaschide (37), it is not a little interesting to see that 'psychology' is the term selected to characterize the treatment of the varied material relating to the hand, which is regarded as the most important organ of movement. In his review of our knowledge of some aspects of fatigue, Offner (23) notes the limitations and unreliability of the objective methods of measuring the mental factors in fatigue which tend to overcome it and secure efficiency in effort. Yoakum (41) has also devoted an unusually large amount of space to an historical and critical survey of fatigue. Without the motive of stimulating research, Partridge (25) has assembled material and suggestions from psychology and anthropometry, in a form designed to simplify the practical study of individuals. One should note the list of several hundred English words (pp. 106–111) which are descriptive of the mental traits in which individuals differ. Although offering less technical material, Rowe (31) has presented a fuller statement of the psychology of habit than we have had hitherto, and hints at new lines for experimental inquiry. Binet (4) has prepared 'a balance sheet,' setting forth some of the results of studies on the sensory, intellectual and moral life of children. The first installment of Marie's (21) rather elaborate attempt to set forth the advance of psychopathology is to be credited to this year. While denying the mental unity of mankind and holding that social life is not the same as individual life, Lévy-Bruhl (20) examines some phases of primitive culture in search of the prelogical types of mental action. Davis (12) devotes attention to a critical examination of current social theories, and concludes that the social mind is only "a mass of common beliefs, sentiments, and determinations, possessed by the individuals of a group with the added consciousness that the other members simultaneously cherish them."

In the wealth of the remaining material, note can be made of only a few contributions. By her able handling of the origin, motives and contents of two hundred and sixty autobiographies, Burr (6) has turned a large bulk of literary interest over to psychology. The

'association lexicon' arranged by Kent and Rosanoff (18) on the basis of free association tests should prove useful in a field wider than that in which it originated. A particular enrichment of procedure and results has been made by Gamble (15) in applying the reconstruction method to memorizing scents, colors and nonsense syllables. By attempting to "determine the ways in which reasoning has grown out of the simpler mental operations," Pillsbury (28) adds to the psychological reconstruction of logic which has been in progress for some years. In continuing his contributions to the experimental study of action consciousness, Ach (1) shows that volition adds something in overcoming old habits of association and seems to arouse characteristic feelings which are proportional to the intensity of the will attitude. Dearborn (13) has usefully arranged his observations of the order of developments in one child. The increasing serviceableness of functional psychology has been strikingly shown by King (19), who contends that, by treating religious phenomena exactly as other facts of experience, it is possible to show how the appearance and development of the religious consciousness has followed the laws of all other mental attitudes. The literature of instinctive behavior has received a rare addition in portions of Wheeler's exhaustive monograph (38).

One may continue to turn to the *Psychological Index* (29) in the confident hope of finding an appreciably definite estimate of the tendencies which are operative in the literary output. By reference to the table below which presents a general comparison of the two preceding years one's attention is struck by two facts. In 1909 there is a decrease of over fifteen per cent. in the total number of references listed in the previous year. There is no possibility of determining whether this is an absolute decrease or whether this falling-off is entirely attributable to the "package of slips containing some of the titles collected by our German collaborators which was lost in the mail" (Compiler's Note). This loss does not, however, effect the relative ranking of the chief topics in the science employed by the *Index* for the purpose of classifying this rich and varied bibliographical material. The ranking order of these ten rubrics remains identical with the order that obtained in 1908. Inasmuch as a decrease occurs in each topic, one cannot refrain from asking: Do we here have an indication of the attainment of a temporary working balance among the dominant interests of psychology? There are considerations relative to the probable rates of scientific advancement which would lead one to regret such an attainment in any science.

A certain phase of the current welfare of the science in America is shown by the record of the doctors' degrees conferred by universities which is kept by *Science* (**32**). Psychology is one of the thirty-two subjects in which the three hundred and fifty-three degrees were conferred in 1910. Twenty degrees, a number greatly above the average for this science since 1898, were bestowed for dissertations on psychological subjects. Three-fourths of these were conferred by three universities, Clark (nine), Cornell (three), and Harvard (three). Psychology still ranks fourth among the sciences which lead university students to continue their studies to a satisfactory conclusion. This science, interestingly enough, also ranks as the seventh of the thirty-two subjects credited with the doctorate.

1908.		1909.	
No. of Titles.	Rubric.	No. of Titles.	Rubric.
782	Genetic, individual and social psychology.	739	Genetic, individual and social psychology.
646	Philosophical implications of psychology.	512	Philosophical implications of psychology.
616	Sleep, trance and pathology.	512	Sleep, trance and pathology.
461	Sensation.	358	Sensation.
337	General.	322	General.
290	Anatomy and physiology of the nervous system.	277	Anatomy and physiology of the nervous system.
189	Conation and movement.	128	Conation and movement.
132	Cognition.	122	Cognition.
49	Conditions and relations of consciousness.	69	Conditions and relations of consciousness.
30	Affection.	28	Affection.
3,532		3,067	

From one point of view this slight indication of the ability of psychology to maintain its scientific and educational position is partially confirmed by Cattell's recent report on the state of scientific efficiency in America (**9**). The three universities mentioned above as conferring most of the degrees are among the five universities (Chicago and Columbia being the others) in which psychology is a strong department, the degree of strength being measured by the presence of at least four of the eminent scientists of the one thousand. On the other hand, this unique study of the psychology of scientific performance reveals a rather discouraging state of affairs among us. Psychology stands only fourth from the bottom of the list of the twelve sciences which have the strongest departments in our universities, a situation hardly to be suspected from the relatively high ranking the work of the science has been holding as indicated above.

That psychology presents widely related as well as closely compacted problems is interestingly declared by some forms of associational interests that appeared during the year. The Boston (1909) meeting of the American Psychological Association was noteworthy for the special report on the methods of teaching the subject to normal school and college students.

One should not overlook the first congress of Polish Neurologists, Psychiatrists, and Psychologists, held in Warsaw, in October, 1909. While revealing some opposition to the laboratory procedure in the science, the program brought forth a commendable list of special psychological topics. On December 27, 1909, the International Committee on Educational Psychology (originally planned by Binet in 1905) assembled in Paris. On the same day there met the committee having in keeping the plans for the proposed International Congress of Paidology. In March, 1910, there was launched at Nüremberg an international organization, under the presidency of Jung, for promoting the interests of the Freudian psychoanalytical method in psychology. The same days during which the Experimental Psychologists' meeting was held at the Johns Hopkins University (April 19–21), the Fourth Congress for Experimental Psychology was being held at Innsbruck. Early in May the American Psychopathological Association, under the presidency of Prince, was formed in Washington, D. C. Late in June a joint meeting was held in London, including the Aristotelian Society, the British Psychological Society, and the Mind Association. Similarly, meetings of psychologists interested chiefly in the educational aspects of the subject were held in the spring in Iowa, in Minnesota, and in New York.

The Journal of Educational Psychology (16) made its appearance at the beginning of the year, and has set forth anew the central position the science may occupy by attracting a strikingly wide range of interests. It is to be a "clearing house for the exchange of information upon all that concerns the relation of psychology to education." The reorganization of special tasks to be undertaken by the PSYCHOLOGICAL BULLETIN, perfected within the year, is especially significant of the rare diversion and complexity which are now accredited to the activity in psychology. A series of psychological manuals for the general reader, under the editorial supervision of Jastrow, has been announced under the heading of the *Conduct and Mind Series*.

In the deaths of John A. Bergström, Borden Parker Bowne, Noah K. Davis and William James, of America, and Paul Mantegazza,

of Italy, this year has led us to appreciate anew the rich services which had been rendered by these laborers to the science in the fields of experimentation, its bearings upon philosophy and its reconstruction, and in its relation to anthropology. Our science has probably received as an indelible stamp the distinctive human impress made upon it by the labors of William James, who, in 1903, shared with Simon Newcomb the rare mark of standing first on the list of the one thousand leading men of science in America (10). By universalizing psychology, he so achieved the unusual in the history of intellectual attainment as to warrant one in believing that the future will regard him as the greatest psychologist of all time.

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HISTORICAL CONTRIBUTIONS

BY PROFESSOR I. WOODBRIDGE RILEY

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The contributions for the year range from the Platonists to the pragmatists. Cushman (1) presents a clear account of the psychology of Plato, Aristotle and the Stoics, but fails to show how the doctrine of representative perception in Democritus was modified in

the non-materialistic compromise of declination made by Lucretius. Werner (6) gives a well arranged and thorough textual study of Aristotelianism in relation to Platonism. Under the four heads—Reality, Mind, the Good, and God—are included such psychological topics as form and matter, soul and body, thought and desire, and the soul of the world. Of especial interest is the treatment of the Aristotelian metaphysics as an introduction to psychology; the splitting up of the dualism between spirit and reality by an appeal to the subconscious; the transfer of the grounds of a theory of pleasure from psychology to ethics and the distinction between practical and theoretical intellect. The work aims to mark the exact delimitation between the two writers and to prove that Aristotle was more inclusive than his idealistic master because of his return to the Ionian physicists.

For medieval psychology Wulf's volume (7) offers an able and cautious exposition. In the relation of soul and body thirteenth century scholasticism held to hylomorphism and the theory of the *spiritus physicus*, and while renouncing Plato and St. Augustine in its inquiries into the composite nature of the human being, agreed with them in establishing the spirituality of the soul. It was, however, regrettable that the leading scholastics were led astray by the distorted interpretation of the *species sensibilis* of Aristotle's commentators, for this supposed transformation of a material effect into an immaterial one uproots the very foundations of scholastic spiritualism. A similar false interpretation is in danger of being made by many modern historians who find a proclamation of the primacy of the theoretical reason in the Thomist theory and in the Scotist and Occamist theories an affirmation of the primacy of the will; Kant's primacy of one faculty over another is a formula which may not be transported without changing its meaning.

Wulf's interpretation of the old scholastic psychology is one thing, his defence of the new is another. The latter, he claims, "has found in the medieval teaching a most appropriate framework of broad leading principles for the interpretation of the latest facts in connection with unconscious mental states, with cerebral localization, with the proper and common sensibles, and especially with the objectivity of our muscular and tactual sensations." These claims may be large, but, as is shown by an interesting account of the text-books and teachings at the Belgian institute, the influence of the new scholasticism of Louvain is not small.

Coming to the 'father of modern psychology,' Kerstiens (2) attempts to meet the objections commonly raised as to Descartes'

ambiguities and inconsistencies. First, the soul is spiritual and not corporeal; though it fills the whole body, it is especially located in the pineal gland which, as a mathematical point, is considered to be without extension. The soul has two chief activities, thought and will, but this does not mean a separation of its substance, but the unity of a substance which is not the mere sum of several operations; hence this new view precludes any approach to psychophysical parallellism. As to free will and the concourse of the duty, Descartes is not so successful in preserving the former apart from the domination of the latter. He hardly foresees the consequences of his doctrines as they issued in the occasionalism of Geulincx and the pantheism of Spinoza. But he elsewhere defends the autonomy of the will in his principle of the counterbalancing of emotion by cognition: "Nullam tam imbecillam esse animam, quae non possit, cum bene dirigitur, acquirere potestatem absolutam in passiones."

In reaction against Cartesianism, Poyer (3) describes Cabanis as opposed to Descartes' dualism and also striving for a science free from any relation to metaphysics. With him causality is nothing but an expression for order of succession, and general truths not absolute but the result of observation. Denying dualism Cabanis is nevertheless no more a materialist than a spiritualist; thus to him neutral states, like cœnæsthesia, belong equally to physiology and psychology. Further the first element of psychic life is not the pure idea, nor the image, but is ideo-motor. This was a new idea of the unconscious and of personality and transcended Leibniz' theory of the 'petites perceptions,' for there may be in each system a sort of partial ego and this central ego may be ignorant of the thoughts and volitions of the secondary ego. Cabanis here approaches the automatism of Pierre Janet and is a precursor of genetic psychology, since his studies of fœtal life and the crises of puberty are made without reference to a finalistic interpretation. Finally his being a functional as against a structural psychology, he differs from the school of Condillac and approaches that of William James and Henri Bergson. In short, according to Poyer, Cabanis is anti-intellectual, positivistic and evolutionary.

The report upon Heymans (5) offers a brief but suggestive programme. According to the Dutch scholar the nineteenth century having been one of scientific method, of positivism, and the study of objective phenomena, has tended to ignore the subjective consciousness.

On one side is an enormous acquisition of scientific and technical

riches, on the other and opposed to the satisfaction of the material needs of man, there is a lack of spiritual satisfaction. The acquisition in exact knowledge is not counterbalanced in the knowledge of the individual. Man finds himself a problematic and strange personality; there is a complication in his intellectual life because he is stripped of his traditional ideas; especially has a loss of religious feeling led to moral depression. Hence arises the triple need of man to make peace with himself, his kind, and life in general. The question is therefore, can the psychology of the future offer the *vis mediatrix naturae*. For the improvement of ourselves and others three factors are important in the evolution of humanity—the succession of generations by death and birth, the selection of the better individual for the perpetuation of the species, and the inheritance of psychic qualities.

Against the charges that experimental psychology cannot hold its men; that its problems are not large enough to satisfy our intellectual demands, Titchener (4) contends that the past decade has shown the contrary. In this time psychology has leaned, very definitely, towards application, has been laid out and aligned with reference to practical and social questions. Take the three fundamental topics of sensation, affection and attention. In (1) sensation the most striking fact to record is the revival and extension of Fechnerian psychophysics and the revival of experimental interest in the organic sensations. (2) In affection we have transcended that pseudo-Darwinism which offers a facile teleology in place of scientific explanation. (3) In attention there has been the revival, with good promise of success, of the attempt to measure the attention, to give quantitative expression to distinguishable degrees of clearness. As to that mixed medley of formations which is included under the general term (4) perception, the Austrian school have made notable contributions but in the American biological interpretations there has been a tendency to substitute final for efficient causes, and general considerations for exact psychophysical determination. In the cognate subjects of (5) recognition, memory and association we may aim at a psychology, a psychophysics, or an applied psychology. Of these three the two latter aims have taken precedence, namely in tracing and measuring the action of the reproductive and perseverative tendencies, and in laying down rules for the training and the economical use of memory. In respect to (6) action, the technical or psychophysical aspect of the experiment has been overshadowed by the psychological, *i. e.*, the method of systematic ex-

perimental introspection. One cannot say as much for (7) imagination. Whether we begin with the elementary process, the image, or regard imagination as the general name for a group of typical formations, as a concept coördinate with memory, we must surely say that experimental psychology is to-day hardly over the threshold of the subject. Of the more complex (8) affective formations we can say but little until we have an assured psychology of feeling. But in regard to the problem of (9) thought, this is a single line of investigation that, more than any other, has characterized the decennium. Here is a new departure in experimental psychology; concept and judgment and inference, the last refuge of the rational psychologist, have been ranged alongside of sensation and association, introspectively analyzed and made subject to the chronoscope. Finally, as to further extensions of the experimental method, the psychology of the abnormal has failed somewhat of its theoretical promise, æsthetics has had a considerable body of results, while the rapid growth of an experimental psychology of the lower organisms has been manifest in the invasion of our laboratories by various unwonted forms of animal life.

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GENERAL STANDPOINTS; MIND AND BODY

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Prominent among current discussions of the relation of psychology to the other sciences are certain attempts to deny its legitimacy or to minimize its scope. According to Duprat (6), psychology is in no wise essentially distinguished from the physical sciences; "we

are an integral part of a world in which there is nothing which is not extended," and the psychic fact is contrasted with the biological fact merely as being "perceptible through internal . . . not through external organs." Professor Alexander (1) does not go so far as this, but he limits psychology to the study of conation and feeling-processes on the ground that sensational content is identical with physical object.[1] On the basis of this conclusion, William Brown argues (4) that psychology is not, properly speaking, a science. It could become a science, he says, only 'by reduction' of the conative processes to the "common terms of space and time in which physics has found it possible to work, *i. e.*, by the measurement of external effects." For general, introspective psychology, Brown would therefore substitute nerve-physiology and *Begabungslehre*, the study of 'mental abilities' (for example of 'sensory discrimination in sight, learning, etc.,' and of 'memory for different classes of facts'), on the ground that these 'abilities' are 'measurable in terms of their effects.'

In opposition to views such as these, W. H. Winch (11) argues in some detail that the translation of psychic phenomena into 'terms of nerves, centers, and nervous paths' is a case of 'passing from what is known to what is guessed'; and that "present physiological and physical schemata are unduly simple." The claim of psychology to rank as independent science is well set forth also by two comparative psychologists: Lloyd Morgan (8) insists that "mental happenings . . . afford material for scientific study." Robert M. Yerkes (12) argues that we should "deal with psychological objects thoroughly . . . instead of devoting most of our time to premature attempts to correlate physiological and psychological phenomena." Yerkes bases his paper on the results of an inquiry addressed to twenty prominent biologists concerning their views of psychology. He distinguishes three types of doctrines; first, the conception of psychology as part of physiology; second, the conception of psychology as part of metaphysics; third, the conception of psychology as a science fundamentally distinguished from biology. The main aim of Yerkes is to establish this third conception and he argues it from the following considerations: The material of psychology is not 'essentially different' from that of the physical and biological sciences; rather, it examines 'the same objects,' but from the subjective not

[1]This statement is made on the authority of Professor Alexander's paper cited above (1). Through delay in the arrival of the last volume of the *Proceedings*, I have been prevented from reading his later paper, On Sensations and Images, *Proceedings of the Aristotelian Society*, 1909–10, **10**, 1–35.

from the objective point of regard. The methods of psychology, also, are those of the sciences: it observes 'under natural and experimental conditions'; it 'strives for quantitatively accurate descriptions of its objects'; it presents, even if 'less satisfactorily' than the physical sciences, 'verifiable accounts of its objects.' Finally, Yerkes insists, psychology explains its phenomena causally, since "from the psychological point of view as well as from the physical, we observe a series of phenomena in which definite sequences are discoverable, and in terms of this apparently necessary arrangement of our objects we explain them." "In physics," Yerkes continues, "this uniform relation of phenomena is called physical causation; in psychics it is called psychical causation."[2]

It is evident that the attempt to relegate psychology to its proper place involves at least two problems—the question of the subject-matter of psychology and the question of the relation between psychical and physical, mind and body. For example, the conceptions of consciousness put forth by the late C. L. Herrick (**7**) and by H. H. Bawden (**2, 3**) inevitably subordinate psychology to biology and to physics. According to Herrick, "the various degrees or grades of consciousness are expressions of successively higher forms of the coördination of forces" (p. 28); and individuality is a 'self-centred or vector' activity. Professor Bawden defines consciousness as "simply the tensional activity or readjustment process which takes place in the universe at one point or another in what are called organisms wherever cosmic energies are brought into interaction in certain definite ways," and says that "a personality has the same sort of individuality that we find in a solenoid or a gyroscope." Such descriptions are, obviously, valueless to those who hold that 'force' and 'cosmic energy' and 'activity' are less immediately known than consciousness.

More important to psychology than these semi-metaphysical attempts to define consciousness in terms of the physical are the teachings, about the relation of mental to physical, of those who admit psychology as independent science. For however heartily one agree with Yerkes that "it is more important for science to advance our knowledge of bodily and mental processes than to speculate concerning their relations," and however emphatically one deprecate the title *Mechanik des Geisteslebens* for Verworn's brilliant lectures on the physiological conditions of consciousness (**10**) it is still obvious

[2]Cf. G. M. Stratton, Toward the Correction of some Rival Methods in Psychology, Psychol. Rev., 1909, **16**, 73 and 74; and M. W. Calkins, cited below (**5**).

that the psychologist cannot rightly ignore the apparently close relation of psychic to biological phenomena. In the first chapter of *A Text-book of Psychology* Titchener (9) adopts what is virtually the monistic and Spinozistic point of view of Ebbinghaus's *Grundzüge*. He asserts (*op. cit.*, p. 2) "that all the sciences . . . deal with some plane or aspect of human experience"; and then he distinguishes (pp. 6 ff.) the material of physics, "as altogether independent of any particular person," from that of psychology which is "altogether dependent upon the . . . experiencing person," which, indeed, "goes on only when some one is there to have it." Physical and psychical facts, thus defined, are, he teaches, two 'parallel aspects of the same world of experience.' It is important to notice the difficulty inherent in this conception as Titchener carries it out. As has appeared, the distinction between 'physical' and 'psychical' has been made by the assumption of an 'experiencing person.' To the obvious question: what is this 'person,' 'some one,' or 'I,' with reference to whom the psychical is distinguished from the physical, Titchener's reply, a few pages further on (pp. 10, 16), reads: "the phrase 'experiencing person' means the living body." But the 'living body' is a physical object, and therefore this identification of 'person' with 'living organism' really reduces the psychical to the physical, destroys the parallelism, and conceives mental phenomena as 'dependent' aspects of bodily processes.

In the paper already cited Morgan takes a radically different view. "Physiological events," he holds (p. 5), "form part of the same continuous train" as "mental happenings." Their relation is that of antecedent and consequent. There are "no scientific grounds for arguing that brain process is the antecedent . . . (and therefore the cause) of mind process, or *vice versa*." In a word, both interaction and parallelism are unnecessary and make metaphysical implications.[1] The 'subject-matter' of the field of psychology is thus 'of a wholly different order' from that of the physical sciences; but it can only "be unified and rendered consistent for scientific interpretation by introducing connecting links which lie outside."

It is interesting to observe the compatibility of psychological dualism, as working hypothesis, with any philosophic theory. The view of Morgan (a realistic monist in philosophy) is also the standpoint adopted by the writer of this notice (a personalistic monist

[1]This statement must be construed as applying only to the form of interaction which treats consciousness as metaphysical cause. Morgan's own view supposes interaction in the sense of mutual relation.

in philosophy) in *A First Book in Psychology* (5). This book is based on the conception of consciousness, as a relation of self to objects[1] (distinguished as personal or impersonal, public or private, external or 'internal'). For purposes of classification and explanation, psychical phenomena are conceived also—by the psychologist not by the experiencer—as related or linked with regularly preceding, accompanying and following nerve-excitations and bodily reactions.

This brings us to the problem of the subject-matter of psychology viewed as independent science—in a word, to the problem of the nature of consciousness more narrowly regarded. Titchener writes, as usual, from the standpoint of the so-called structural psychology—better named, 'atomistic' or 'idea' psychology, since analysis is a method common to all serious forms of psychology. This view of consciousness or mind, as sum total of mental processes, or series of mental happenings, is held by Morgan also, who argues in explicit opposition to the conception of consciousness as self. According to Morgan the self, or 'conscious ego,' is an 'active agent or producing cause' and, as such, an object (in his view, a justified object) of metaphysics. Morgan speaks, to be sure, of the 'experiencer'—for example, in his descriptions of 'the water-hen as actual and potential experiencer.' He does not, however, follow Titchener in identifying the experiencer with the organism. (It is better, he says (p. 15), to say 'the organism has psychophysical dispositions' than to say that the 'organism has impressions.') And he asserts (p. 14) that "to know that one has an impression implies a thinker or experiencer." But, curiously enough, to avoid what he calls the metaphysical conception of the self as experiencer, Morgan has recourse to the metaphysics of the associationists, concerning "the perceptual subject" as "the revived experience which assimilates or possesses the new impression."

The writer of this notice regards psychology after a radically different fashion, as science of the conscious self in relation to its environment. This self, it is contended, is not the ultimately real self, the 'determining agency' of the metaphysicians but a self immediately realized as constituting every conscious experience however elemental. The book already cited (5) attempts to lay stress indirectly or directly on the following considerations which favor this view: (1) All experiences are incompletely described, and certain experiences—

[1]The core of truth in Professor Alexander's discussion is the recognition of the psychologist's object. The main objection to his teaching is that he does not carry the dualism far enough.

recognition and emotion, for example—are very inadequately described without reference to a persistent and unique self. (2) This 'self-psychology' includes a complete structural analysis—that is, it is as easy to treat the experiences of a self as sensational, affective, etc., as it is to analyze a mental process into sensations, affections and the like.[1] (3) All psychologists, however opposed in theory to the recognition of a self, none the less implicitly treat psychology as science of the self, or experiencer.

The clearest result of the reading of these divergent discussions is, thus, the fear that we are yet far off from the day when, in the words of Yerkes "a certain group of presuppositions" shall have been "definitely and heartily accepted by the great mass of . . . workers" in psychology.

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[1]For the brief expression of an opposite view, *cf.* the exclamation of Duprat, cited above: "*le 'je' est vide de contenu.*"

TERMINOLOGY

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Claparède (1) reported to the last International Congress of Psychology a plan for the revision of psychological terminology. In his opinion the desiderata include the compilation, determination, and authorization of convenient terms now in use; choice among rival terms or rival meanings which attach to the same term; the determination or coining in each language of equivalents to terms recognized in others; coining of new terms to designate new things, or where existing terms are unsatisfactory; and the creation of time-saving international symbols for the most common terms. The author advocates the following principles: (1) Words relating to the same thing should so far as possible be identical in all languages; the use of Latin and Greek roots is recommended. (2) Terms established in one language should be translated literally into the others. (3) Preference should be given to words already in use over neologisms. (4) Each object or concept should be given an individual, appropriate name. (5) In coining new terms rules already applied in analogous cases should be followed. (6) For experimental technique and phenomena peculiar to psychology, special and characteristic terms should be adopted rather than ill-defined, popular terms. (7) The reform should begin with terms relating to methods, technical procedure, etc., rather than those which imply some hypothesis or doctrine.

Claparède makes several pertinent recommendations—*e. g.*, French names for the psychophysical methods; E.S. and E.T. for 'space error' and 'time error' in all languages; P for subject (patient) and A for experimenter (assistant); N and V for normal and variable (or comparative) stimulus.

The same author (1) reports the propositions presented to the Congress by Baldwin. Although devised independently, the two agree substantially. Baldwin proposed, furthermore, that technical terms having a recognized meaning in one language should not be used in another language with a different meaning; nor should they be employed in their own language with other meanings unless accompanied by some determining word. The Congress voted that new words should be so constructed that they may be carried over with slight change into all the principal languages. A commission of four was appointed to present a definite scheme to the next Congress.

Titchener (4), while offering no striking innovations in terminology, is unusually exact and thorough in his definition and demarcation of terms. The relations between perception (364), idea (376), meaning (367), apperception (366), memory image (416), and image of imagination (416) are brought out. Sentiment is differentiated from emotion (499). 'Conscious attitude' is treated exhaustively (505–521). The author finds no compelling reason for the retention of the terms apperception (366), perseveration or perseverative tendency (401), and conation (468).

Of interest to genetic and social psychologists is Rosenthal's discussion (2) of the relation between 'tendencies of development' and 'laws' in social phenomena.

In ethics Sissons (3) proposes the term 'catholism' (adj. 'catholistic') in place of 'utilitarianism' and 'universalism,' to denote the balance between egotistic and altruistic impulses.

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TESTS

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The greater part of the publications upon tests during the past year has been related to the Binet-Simon 'Measuring Scale of Intelligence.'[1] The article by Huey (7) gives a partial translation of the Binet-Simon tests and of the directions for using them, and urges that they be standardized with normal American children so that they may be used intelligently in grading abnormal children.

The standardization of these tests by applying them to normal children has been attempted by Decroly and Degand (5). These investigators applied the tests to 45 children of a day school extending in age from 2 yrs., 7 mo. to 12 yrs., 8 mo. There were 37 between the ages of 4 and 9 inclusive. The authors publish the results with

[1]Binet et Simon. *Année Psychol.*, 1908.

each child in some detail so that it is possible to arrive at some results which they do not themselves state. It appears that on the basis of the tests, using Binet's method of determination, only 2 of the pupils were retarded and these each 1 year while 5 tested normal and 38 were advanced. The advancement ranged from ½ yr. to 4 yrs. and amounted in all to 69½ yrs. In brief, assuming that the tests were made according to Binet's directions, either the tests are on the whole too easy or the children tested by Decroly and Degand are more mature for their age than those which Binet used in constructing the tests. The authors themselves make some criticisms of certain of the tests. They conclude that 12 of them are too easy and 3 too hard. They criticize 4 tests because they involve mechanical processes rather than intelligence, and 4 others because they depend too much upon the social *milieu* of the child. Others again, to the number of 7, indicate merely erudition. They would suggest changes, then, in 30 out of the 65 tests. Finally they suggest that the tests for immediate memory be more finely subdivided so as to furnish one for each age from 7 to 12 yrs.

The Binet-Simon tests were further tried out, though not very accurately, by Miss Johnston (8). She tested 200 school girls of Sheffield, England, most of whom were aged 7 to 13 inclusive. Of these 140 could not meet the Binet test for their age. Most of these however—the author does not say how many—could pass enough tests of their own age and beyond to bring their standing up to normal according to Binet's corrective formula. There were 26 who could meet the tests beyond their age. Considering those who were advanced more than one year we have a total of 39 years advancement. The interpretation of these results is doubtful, but they are not in accord with those of Decroly and Degand.

Goddard (6) grouped the 400 children of the New Jersey Training School on the basis of the Binet-Simon tests and then tested the grouping on the basis of certain other criteria. He first assembled the executive heads and teachers of the institution, read the names of the children belonging to each group to them and asked them if any children were in their judgment misplaced. Though certain preliminary objections were expressed they were in each case overruled and there was finally unanimous agreement that the children were correctly placed. The order of grouping was also compared with the results from tests with the form board, ergograph, ataxiagraph, etc., and with the accounts of the attendants, teachers and officers of the general ability of the children to peform actions of

various sorts—work, play, etc. It was found in each case that the grading by the Binet tests agreed with that reached by the other means.

Lipmann (9) has made a comparison of the distribution of ability about the normal made on the basis of Binet's tests with three independently constructed curves of distribution, viz., the theoretical curve of Francis Galton and the curves determined on the basis of experimental tests by McDougall and Bobertag. (Unfortunately the author does not give the sources of his data.) The author has drawn the four curves in one figure which supports his statement that there is a striking similarity.

Experiments with a newly devised test are reported by Ries (12) The object of the test is to measure the ability to establish a logical relation (cause and effect) between two concepts. The assumption is that this ability will show a high correlation with general intelligence. The method is to determine the degree to which the associative memory of pairs of words between which a relation of cause and effect exists differs in different children and to grade them on the basis of their efficiency of recall of the second word of the pairs when given the first one. That this is not a test mainly of mechanical memory was shown by a check experiment with nonsense syllables. The correlation between the two kinds of recall was only .40, with p.e. .11. The correlation of the standing in the new test with class standing was worked out for five classes of pupils ranging in age from 11 to 14. The coefficients for the five classes were .59, .85, .89, .86 and .90. A variation of the method consisted in giving the pupils a series of substantives and requiring them to find words which express the effect of that which the first word denotes. Excellence in this test gave still higher correlation with class standing. The Ebbinghaus Completion Test gave in general a lower correlation than the tests devised by the author.

Binet (1) contributes a report of experiments on the correlation between intelligence as indicated by school standing and certain physical characteristics. He chose for examination the size of the head, the stigmata of degeneracy, physiognomy and the form and appearance of the hand. The characteristics in question he does not find to be suitable subjects for tests, but the first two may in certain cases furnish confirmatory evidence.

Meumann (10), in a critical article, analyzes the different forms of mental tests and gives suggestions as to the forms which are most reliable. In view of the fact that we have but little knowledge of the correlation between particular mental processes and general intelligence the author holds that the examination of general intel-

ligence should be made by means of a series of tests varied in character rather than of single tests. On the other hand the determination of the degree of 'higher intelligence,' or intelligence in the narrow sense, may be made by means of tests specially adapted to this end. These would include tests of the ability to make abstractions, to keep in mind a goal of thought, to command readily and in ample number ideas appropriate to this goal, etc.

Paton (11) describes the method of selecting 25 boys from a school of 250 for the award of scholarships. The method consists in a written report of a passage read just previously, and in a personal, individual interview with each boy.

Brown (3 and 4), describes a method of correlating excellence in different subjects on the basis, not of the standing in the subject as a whole, but in certain parts of the subject, each of which involves chiefly some one mental process. The author has applied this method to the work of 50 boys in arithmetic, algebra and geometry and reaches what he regards as evidence of the value of the method.

For reviews of Burt (2) and Whipple (13), see the October number of the PSYCHOLOGICAL BULLETIN.

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SPECIAL REVIEWS

TITCHENER'S PSYCHOLOGY

A Text-book of Psychology. EDWARD BRADFORD TITCHENER. New York: The Macmillan Co., 1910. Pp. xx+565. Price $2.00 net.

This new work by the dean of American empirical psychology, written to take the place of his earlier *Outline of Psychology*, is a systematic treatise which is perhaps most closely comparable, by reason of its clearness, its maturity of judgment and the candor and conviction of its statement, with the unfinished *Grundzüge* of the late Professor Ebbinghaus. It shows the same respect for empirical findings as does the excellent text-book of Professor Myers, but a broader and more unifying grasp thereof than does that volume. Professor Titchener's work presents the field of psychology apprehended as a whole by a mind which surveys with apparent ease the parts and the whole together, and surveys these with a considerable originality of vision. Such a work will stimulate both to assent and to dissent, but it is sure to stimulate and to instruct.

To consider, firstly, the use of this volume in the class-room—and it is 'written expressly for the class-room'—it should seem to be a perfectly feasible text for beginners since the elementary facts are there and are clearly and simply stated. But it contains very much that would be wasted on beginners, and that on the other hand more advanced students ought decidedly not to miss. So that, although the items and discussions of a more advanced quality are presented in smaller type and can be omitted with a class of beginners, the volume should seem best adapted to a second course in psychology. Furthermore Professor Titchener presupposes an already awakened interest on the part of the student and makes small concessions to the art of allurement; as is scientifically impeccable, of course, but with an elementary class is pedagogically rather inexpedient, at least in America.

The contents are arranged under these main headings—Sensation (with nine subdivisions), Affection, Attention, Perception (with five subdivisions), Association, Memory and Imagination, Action, Emotion, Thought. Into the general matter and structure of these sections we need not enter, since they are in good part predetermined for any author; while the treatment of certain particular points is here of more vital interest. Firstly, Professor Titchener rejects the

notion of an unconscious (or a co-conscious) mind: although "reference to the body does not add one iota to the data of psychology," yet it "does furnish us with an explanatory principle for psychology; it does enable us to systematise our introspective data. Indeed, if we refuse to explain mind by body,—we must either rest content with a simple description of mental experience, or must invent an unconscious mind to give coherence and continuity to the conscious" (page 40). The unconscious belongs to 'the sphere of fiction.' This principle is consistently adhered to, so that even the 'meaning' of our perceptions, which is ordinarily the imaginal context, the images associated with perceptions, 'may be carried in purely physiological terms'; it has, 'time and time again, no discoverable representation in consciousness' (369). There is, then, nervous action unaccompanied by mental process notwithstanding that mind and body "are not separate and independent things" and "any change that occurs in the one will be accompanied by a corresponding change in the other" (13). Some further explication is here needed to reconcile this statement with the view that mental processes correspond to only certain events within the nervous system and that the gaps in the former series can be explained by reference to the causal continuity of the latter (40). Clearly, however, Professor Titchener would not resort to dissociated or co-conscious mental processes, as is more or less explicitly done by the psychoanalytic school.

Both space and time are made (quantitative) attributes of sensation. The "elementary character of outspread or expanse is the foundation upon which all the forms of spatial consciouness, delicate and refined as they are, have been built up" (303). "The attribute of duration is, in the author's view, primitive and ultimate, like that of spatial extent; it corresponds, in consciousness, to the rise, poise and fall of the excitatory nervous process" (340). Concerning the genetic theory of space-perception by means of sensations of movement, the author says: "To say that space results from the fusion of quality and intensity [of a kinæsthetic sensation], however plausibly the statement be made, is to leave us with a mystery; nowhere else, over the whole range of psychology, does the concurrence of attributes give rise to an absolutely new form of consciousness," *i. e.*, the spatial (338). In short the idea of extent cannot be formed out of merely intensive entities. But this nativism, if so it is to be termed, is adopted along with its consequences—a concession to fact that is all but unprecedented in the annals of psychology—for "if the idea is the idea of a circle seen in the mind's eye, it is round; and if it is the

visual idea of a square, it is square" (17). Nevertheless Professor Titchener does not deny that kinæsthetic sensations play an important part in the perception of space; and they can do this because "the articular sensations are themselves spatial in character" (307). This last point is not given quite enough emphasis, and unless the student gets it he will find it hard to reconcile the above-quoted criticism of the sensation-of-movement theory with such sentences as this, regarding binocular depth: "it seems probable that absolute localization is effected by way of muscular sensations, the sensations aroused by movements of accommodation and convergence" (313); as also with passages on pages 307, 308, and 314. Indeed the reviewer is puzzled to see why, if the author's theory is correct, "the sensations [last mentioned above] are not always discoverable by introspection; the perception of distance may come to consciousness directly" (314). Is it meant that the spatial attribute alone, of the sensations of accommodation and convergence, comes to consciousness?

The interesting topic of sensory blends is considered under 'Qualitative Perception.' It is characteristic of these experiences that the component qualities blend "so that the perception appears simple, or at least unitary"; but also "that the perception may be analysed, under rigorous scrutiny, into a number of elementary processes." The "blending may be so complete as to give us the illusion of qualitative simplicity. But the other fact, that systematic observation always reveals the complexity of the perception, is of no less importance" (349). "The classical instance of the qualitative perception is the tonal fusion" (351). Such blends occur between visceral and dermal sensations: indeed, "the skin may steal from the viscera as taste steals from smell" (184). But the author does not admit (with, for instance, Brentano) that color qualities are such complex perceptions; for, "all colours are equally simple; it is impossible, for instance, by introspective analysis to split up orange into yellow and red" (85, also 61). Yet, as above, qualitative simplicity may be an illusion; and on the other hand many observers, both untrained and trained, report themselves able to analyze introspectively several of the color qualities.

The treatment of affection and attention is substantially like that given in Professor Titchener's *Lectures on the Elementary Psychology of Feeling and Attention*. One addition may be noted. It will be remembered that in the previous volume affections were found to lack the attribute of clearness, so that they were only with difficulty, if at all, amenable to attentive study. It is now found "not necessary

to affective introspection that attention be directed upon the affection. Recent work has shown that, if attention is directed upon the stimulus, the situation, an affective judgment comes directly, of itself the quality and intensity of the affection touch off the corresponding expression or report. Nothing more is required than that the observers shall be fittingly instructed, affectively predisposed. When the predisposition to express the feelings has been set up, it is this that is actualised by the presentation of the stimulus, and that dictates the introspective report. The observer has no impulse to describe the situation in sensory terms, focal as these terms were" (472–473). This may suggest to some psychologists the doubt, whether such predisposition does not after all render the affection focal, that is, bring to it the attribute of clearness.

In the chapter on Emotion Professor Titchener deplores the fact that "we have no systematic description of emotion, taken under standard conditions. In other words, we have no first-hand analytical psychology of emotion; and it is this lack that makes the chapter on emotion in the text-books the dreary reading that it is" (472). "The reason, . . . that our descriptive psychology of emotion is schematic rather than analytical is, simply, that experimental psychology has so far found neither the time nor the courage to take emotion into the laboratory" (473). The James-Lange theory is reviewed, and the conclusion reached "that the sensations of the organic reaction cannot be identified with the emotion. They are, it is true, an integral part of the typical emotive consciousness,—but only because the emotion is, essentially, an affective response to a situation which appeals to the organism's instinctive tendencies" (483). "A group of organic sensations is, after all, a group of organic sensations" (482). "And if an emotion has previously run its complete course, inclusive of the organic sensations, then the inhibition or deferment of the organic reaction, later on, does not of necessity destroy the emotion; the primary feeling, the affectively toned situation, may now, in its turn, mean or stand for the complete emotion" (483). But would this be the case if the emotion had not previously "run its complete course, inclusive of the organic sensations"? The late Professor James believed not; but granted that after the organic sensations had once become associated together, after being reflexly aroused together by a given environmental situation, a recurrence of the situation might arouse a memory of these associated organic sensations without actually exciting them reflexly. Also in the above quotation it should be noted that the case of something 'standing

for' a complete emotion is not the case of there being a complete emotion. However, Professor Titchener admits that the organic sensations are 'an integral part of the typical emotive consciousness,' and Professor James himself indicated a further aspect of emotions: "The man is really hateful; the action really mean; the situation really tragic—all in themselves and quite apart from our opinion" (The Place of Affectional Facts in a World of Pure Experience). The emotion is partly comprised, then, in the structure of the situation itself. Professor Titchener gives some interesting introspective analyses of emotions, affective transfers and illusions: and mentions in passing, the 'law of dynamogenesis.' "Still it remains doubtful whether every excitation that corresponds to sensation or image tends to a motor conclusion." The view, "that the nervous system is a system of conduction, a sort of glorified telephone exchange, is in the author's opinion wholly inadequate to explain the phenomena of mind. The theory of conduction, with obstacles or easements between cell and cell, must, he believes, be replaced by a theory of intracellular change, of change within the cell-body;and if this is the case the cortex may be regarded rather as disjunction of the reflex arc than as a switchboard for the manifold connection of afferent with efferent process" (489).

Perhaps no portions of this volume are written with more care and penetration than the chapters on Action and Thought. Both chapters deal essentially with the range of phenomena that has come to be known as the 'thought-process,' and both contain a remarkable wealth of concrete introspective analysis; and the treatment is very different from that found in the same author's *Lectures on the Experimental Psychology of the Thought-Processes*. The most important element in 'action' is the 'determining tendency,' with its complicated relations to consciousness. Voluntary action is 'selective action' or action subject to more than one determining tendency; it results "from a conflict of impulses, just as secondary attention results from a conflict of primary attentions" (459), and these several determining tendencies (in marked contrast with the Lotzian view) may or may not be present in consciousness. "Determinations may be carried in very fragmentary terms, and a wide range of reflective consideration may be packed into a conscious attitude" (460). The following definition is well worth noting: "A suggestion is any stimulus, external or internal, accompanied or unaccompanied by consciousness, which touches off a determining tendency" (450). In Thought the author attacks the imageless thought-element and the imageless

relation-element, but he makes room for a 'conscious attitude' that is more or less analyzable into imagery. The following passage will be taken by some persons as a concession to Bühler, by others as an explanation of how Bühler and those who share his opinions in this matter have come to make their mistake: "It is, then, conceivable that the imageless relations of the text [there considered] mark a half-way stage between kinæsthetic set and unconsciousness; that, in certain individuals, a faint glow of consciousness still plays about excitatory processes which, in other individuals, are altogether unconscious. It is more probable that a systematically controlled introspection will, in all cases of consciousness, reveal the imaginal character of the feeling of relation" (514). Attitude is generally analyzable into imagery of one sort or another, but it appears "that certain images are constitutive of the attitude, and that certain other images are secondary and irrelevant" (519). To the reviewer there seems to be an ambiguity as to the relative importance, for the attitude, of imagery and of determining tendency. It is hard to see how a determining tendency can be any kind of conscious image, or how any image can have the dynamic potency of a determining tendency. These two chapters, furthermore, in common with the literature of the thought-process in general, reveal in the reviewer's opinion a fundamental error;—the report of *introspective* consciousness, of consciousness complicated by the additional processes of reflection, that is of recall, analysis and description at the very least, is everywhere mistaken for consciousness. Whereas the consciousness that is not so complicated, which is not introspected on or which is not amenable to introspection, is what others have called the unconscious or the coconscious; and in the literature of the thought-process it is denied to be anything but neural process. This confusion is the source of the 'imageless thought' controversy; of the paradox that mere neural process can at some times accomplish satisfactorily what at other times requires the accompaniment of consciousness; of the predicament which requires us to discover just what change in the nature or locality of neural process causes it to be accompanied by consciouness; and of a host of other bafflements.

But this review has exceeded its limits, and has still barely touched on the numerous important issues that are raised by Professor Titchener's latest work. It is a volume of the first importance, and one which will instruct and stimulate the teacher no less than it will safely guide the student and the lay reader.

Edwin B. Holt.

Harvard University.

APPARATUS

A New Key for Reaction-Time Work. K. DUNLAP. PSYCHOLOGICAL MONOGRAPHS, 10, 1909, 26–37.

This compound key is designed to eliminate errors due to breaking of the current by the finger resting on the key. Experimental proof is furnished showing that this key shortens reaction-time presumably by favoring the withdrawal of attention from touch of the key. Full directions for construction are given.

An Apparatus for the Study of Kinæsthetic Space Perception. J. H. LEUBA. Amer. J. of Psychol., 1909, **20**, 371–373.

This is an elaborate piece of apparatus for control and measuring of the arc movements of arm and leg in either active or passive movement with convenient means for measuring the amplitude and extent of the movement.

Tuning Forks for Tests of Pitch Discrimination. E. B. TITCHENER and G. M. WHIPPLE. Amer. J. of Psychol., 1909, **20**, 279–281.

A full criticism of the principles involved in this apparatus is found in the report of the committee on the standardization of the tests of pitch discrimination, which appears in No. 53 (December, 1910) of the PSYCHOLOGICAL MONOGRAPHS.

A Demonstration Color-Pyramid. E. B. TITCHENER. Amer. J. of Psychol., 1909, **20**, 15–21.

This article contains directions for the making of the demonstrational color-pyramid. In making one it is worth while to consider with this the suggestions 'on the form of the color-pyramid' by Howard C. Warren, PSYCHOL. BULL., **7**, 51–52.

Ein Tachistoscope fur Reizserien. W. WIRTH. Psychol. Stud. (Wundt), 1909, **5**, 268–278.

This is a well-built shutter controlled by an electro-magnet and coil springs for short exposure. It is a very serviceable piece of apparatus but does not give that freedom from noise which is obtained in the use of the Dodge tachistoscope with revolving disk. A simpler shutter with a straight spring is also described.

With increase in specialization of scientific work in psychology it is more and more important that permanent pieces of apparatus should be described in a separate article and not merely as an incident in a piece of research. It is, also, desirable that psychologists should report from time to time improvements and standardization tests of

apparatus which may be of general interest to the directors of laboratories. It is hoped that the section in this bulletin which is now set aside for the review of apparatus may contain in the future very much more extensive and critical accounts of the annual out-put of apparatus. For this reason it is desirable that notices, preferably reprints of articles on apparatus, should be sent to the editor.

C. E. SEASHORE.

UNIVERSITY OF IOWA.

NOTES AND NEWS

At the recent meeting of the American Psychological Association at Minneapolis, Minn., Professor C. E. Seashore (Iowa) was elected president for the coming year. Professor W. V. D. Bingham (Dartmouth) was elected secretary-treasurer.

The American Philosophical Association has elected Professor F. J. E. Woodbridge (Columbia) president and Professor W. T. Marvin (Rutgers) vice-president. Professor E. G. Spaulding continues as secretary.

The Southern Society for Philosophy and Psychology has elected the following officers for the year 1911: *President*, Dr. Shepherd Ivory Franz (Government Hospital for the Insane); *vice-president*, Professor A. Caswell Ellis (Texas). Professor R. M. Ogden continues as secretary.

Professor John Dewey, of Columbia University, is giving a course of six lectures at Smith College on 'The Psychology and Ethics of the Self.'

Professor James R. Angell, of the University of Chicago, will give a course of eight lectures at Union College in January and February on 'Modern Psychology.' These lectures are given on the Ichabod Spencer Foundation which, with an endowment of $75,000 for the department of philosophy, was recently made by Mrs. Catherine Leavitt, of Washington, in memory of her father.

Vol. VIII. No. 2. February 15, 1911.

THE

PSYCHOLOGICAL BULLETIN

PROCEEDINGS OF THE NINETEENTH ANNUAL MEETING OF THE AMERICAN PSYCHOLOGICAL ASSOCIATION, MINNEAPOLIS, DECEMBER 28, 29 AND 30, 1910

Report of the Secretary

The nineteeth annual meeting of the American Psychological Association was held in Minneapolis on Wednesday, Thursday and Friday, December 28, 29, and 30, 1910, in affiliation with the Western Philosophical Association and the American Association for the Advancement of Science. All sessions were held in the buildings of the University of Minnesota. Everything possible was done for the general convenience of all, and the members of the department of philosophy and psychology did much to contribute to the enjoyableness of the occasion by personal courtesies extended to many of the members present.

The Association was represented by about forty of its members, most of these from the west. Members of the North Central Association of Teachers of Psychology were present at several of the sessions.

On Wednesday morning, December 28, at ten o'clock, the meeting was formally opened by the President, Mr. Pillsbury, and the carrying out of the printed program was immediately begun.

Two joint sessions were held, one on Wednesday afternoon with Section L of the American Association for the Advancement of Science, and the other on Thursday morning with the Western Philosophical Association. At the latter session the marked contrast between the philosophical and the psychological usages of the term 'consciousness' came emphatically to light.

On both Wednesday and Thursday evenings, after the addresses of the Presidents of the Philosophical and Psychological Associations respectively, a smoker was given by the department of philosophy and psychology of the University.

The program occupied two days and a half. Abstracts of the various papers will be found below.

At the business meeting, held Friday forenoon, the following business was transacted:

The following officers, nominated by the Council, were elected: *President* for 1911, Professor C. E. Seashore, of the University of Iowa; *Secretary and Treasurer for three years*, Professor W. V. D. Bingham, of Dartmouth College; *Members of the Council to serve three years*, Professor A. H. Pierce, of Smith College, and Professor H. C. Warren, of Princeton University.

On nomination by the Council the following candidates were elected to membership:

Edward Scribner Ames, Ph.D., University of Chicago; George F. Arps, Ph.D., University of Illinois; W. C. Bagley, Ph.D., University of Illinois; Frederick G. Bonser, Ph.D., Teachers College, Columbia University; William F. Book, Ph.D., University of Montana; Eliott Park Frost, Ph.D., Yale University; Robert H. Gault, Ph.D., Northwestern University; Nathan A. Harvey, Ph.D., Michigan State Normal College; Grace Helen Kent, A.M., George Washington University; Linus W. Kline, Ph.D., Minnesota State Normal School; Harvey A. Peterson, Ph.D., Illinois State Normal University; A. J. Rosanoff, M.D., Kings Park State Hospital, New York; Eugene C. Rowe, Ph.D., Central State Normal School, Michigan; Arthur H. Sutherland, Ph.D., University of Illinois; E. B. Titchener, LL.D., Cornell University; Clarence S. Yoakum, Ph.D., University of Texas.

The treasurer's report, audited by the Council, was read and approved.

On recommendation by the Council it was voted to accept the invitation of Dr. S. I. Franz to meet next year in Washington, the Council being empowered, however, to make any change of plan that may ultimately seem advisable.

On recommendation by the Council it was voted that a committee of nine be appointed by the chair to consider and report on the relations of the Association to the American journals concerned with psychological research. The chair appointed on this committee Messrs. Warren, Angell, Woodbridge, Cattell, Sanford, Bell, Yerkes, Prince and Hall.

On motion by Professor Martin it was voted that a committee be appointed by the President to hasten, if possible, the publication of résumés of psychological investigations, and to induce the authors themselves to furnish short abstracts of their results to the various

psychological journals as soon as their papers appear in print, and to induce the psychological journals to make the résumés furnished the basis for reports on such work. (The incoming President referred this matter to the committee on periodicals just appointed.)

On recommendation by the Council, amended by the Association, the following resolutions were voted: (1) that the Association extend a hearty welcome to the Seventh International Congress of Psychology for its meeting in 1913; (2) that in place of the regular meeting in December, 1912, the Association meet in the spring of 1913 in conjunction with the International Congress; (3) that a committee, composed of Messrs. Cattell, Münsterberg, Sanford, Titchener, Watson and Bingham, be appointed to coöperate with the officers of the Congress.

On recommendation by the Council it was voted to make a further grant of an amount not exceeding $250 to the Committee on Tests for the publication of their results, such publications to become the property of the Association. It was also voted that each of the authors of these reports be given twenty free copies of the same.

On motion by Professor Bingham it was voted that the incoming President appoint a committee of three on the 'class experiment' and the 'home experiment,' as recommended in the report of the committee of the Association on the teaching of psychology. (Cf. PSYCHOLOGICAL MONOGRAPH, No. 51, p. 91.)

On motion by Professor Swift it was unanimously voted that the Association express its appreciation of the pleasant receptions given to its members by the department of philosophy and psychology of the University of Minnesota, as well as for the many other courtesies received from the department.

It was voted that the proceedings of this meeting be printed.

REPORT OF THE TREASURER FOR 1910

DR.

To Balance from 1909 meeting	$3,070.71
Dues received from members	221.40
Interest from July 1, 1909, to July 1, 1910	108.41
Petty cash on hand January 1, 1910	.70
	$3,401.22

CR.

Stationery and printing	$82.50
Clerical assistance	10.45
Postage	35.40
Express and telegram	4.60

Travelling expenses (1909 meeting)	21.16	
Appropriation for publication of report of Committee on Methods of Teaching Psychology	150.00	
Distribution of free copies of the above	18.82	
Exchange on checks	.60	323.53
Balance in Union Dime Savings Institution	$2,973.27	
Balance in Fifth Avenue Bank	104.42	3,077.69
		$3,401.22

A. H. PIERCE,
Secretary and Treasurer.

NORTHAMPTON, MASS.,
December 15, 1910.

Audited by the Council.

ABSTRACTS OF PAPERS

President's Address: The Place of Movement in Consciousness. WALTER B. PILLSBURY.

(This address will be published in full in the PSYCHOLOGICAL REVIEW, Vol. XVIII., No. 2, March, 1911.)

The Psychology of Drowsiness. H. L. HOLLINGWORTH.

A report and analysis of the normal drowsiness hallucinations recorded by two observers, during the past two years. The drowsiness hallucination seems to be a 'flashlight' perceptual fusion or complication, and is further characterized by transformation of imagery type; sensory, perseverative and ideal substitution; fluid association on a sensory basis; and by isolation of association trains when they develop. It is accompanied by tendencies toward grandeur and vastness, by rapidly ensuing amnesia, and by absence of symbolism.

(The paper will appear in full in the January number of the *Amer. J. of Psychol.*)

The Projection Method. LILLIEN J. MARTIN.

Series I.—Experiments were made to ascertain (1) whether visual images are projectable into space at will; if so (2) whether when so projected they are amenable to examination, and of such a character as to be advantageously examined; and (3) whether anything is gained by having the images so projected. The reagents were ten members of the Psychological Institute of Bonn and, in part, fifty Stanford University students.

The reagent was seated at a table on which was placed in turn a flower-pot, postal-card of a landscape, etc. As each object was set

on the table, the reagent was directed to place a visual image of the object at its side. If able to obtain the required image, he was asked to give his introspections to protocol, and was questioned on points calculated to throw light on theories regarding the difference between the perception of a real object and its image. The introspections show in general that the reagents were able to project their visual images with no very great difficulty, and to compare them when so projected with the corresponding object.

Conclusions: (1) The projection method is strong in just those respects in which the questionnaire is weak. (2) It enables one to make a comparison as regards strength of impression in the case of different reagents. (3) The standard and that to be compared with it are put under the same environment. (4) By the projection method the object and image can be examined simultaneously. (5)This method does away with the difficulty regarding the time that one may allow to elapse after the completing of the reaction before one begins to put in question the reliability of the introspective reports. (6) The image can be made the same size as the object, and (7) can be placed at the same distance. (8) It can also be kept constant, so far as the reagent's ability to do so permits, for he has a standard to compare it with. (9) Other memory and fancy images are also much less prone to come unbidden and push aside the image under study. (10) The method furnishes favorable conditions for comparing voluntary and involuntary images, and (11) for learning to what extent *Erkenntniss* depends upon visualization.

Series II.—To learn whether the projection method can be advantageously employed in studying the difference between memory and imagination.

The reagents were instructed to project a visual memory and imaginative object side by side on the table. Introspections and questions as before. The method showed itself adequate.

Series III.–IV.—Tests made showed that the method is applicable to the study of illusion, hallucinations, etc.

Series V.—To ascertain whether the projection method can be used to advantage in investigating auditory memory and imagination, —illusions and hallucinations. The results show that some additional precautions are needed in experiments involving auditory stimuli.

An Introspective Analysis of Tactual Phenomena. GEORGE F. ARPS.

The introspections of this paper were obtained from two groups of pressure stimuli. In the first group the pressures were more or

less extended in duration; in the second group they were of momentary duration. In both groups the pressure stimuli were applied successively to the upper phalanges of the index and middle fingers. The standard stimulus, in the first group, varied in duration but remained constant in intensity. The standard stimulus, in the second group, remained constant in duration and intensity. The comparative stimulus remained constant in duration and varied in intensity in both groups.

In group one, the standard pressure was given by means of an especially constructed electro-magnetic pressure apparatus; the comparative pressures were given by means of a pressure-balance with a pneumatic attachment. In group two, both standard and comparative stimuli were given by means of small metal balls dropped from small electric magnets.

The introspections center chiefly about the subjective variations in the intensity of the standard stimulus and the subjective fluctuations of the period of duration of the comparative stimulus.

Summary of results (first group): (1) It appears that the comparative stimuli, under the given conditions, render constancy in intensity, through assimilative effects, in a given norm subjectively impossible. (2) The assimilating elements (comparative stimuli) are most efficient within certain limits of a series, above and below which assimilative effects are minimal or entirely lacking. (3) Greater assimilative effectiveness is found when the normal stimulus precedes the comparative stimulus. (4) There is a noticeable increment in the assimilative efficiencey when comparative stimuli are given in the ascending series. (5) It appears that the assimilative efficiency is proportional to the duration of the norm and of the comparative stimulus. (6) That some momentum in a series of comparative stimuli appears essential to assimilation is shown by the large number of fluctuations in the standard stimulus at the middle of any series.

The fluctuations noted above pertain to the intensity of the standard stimulus. Fluctuations of a somewhat different order are recorded for the period of duration of the comparative stimulus. It is believed that the normal stimulus acts assimilatively on the comparative stimulus with reference to its duration as does the comparative stimulus on the normal stimulus with reference to its intensity.

Summary of results (second group): The results are practically negative. Only 16 cases of norm fluctuations are recorded among

a total of 1,622 experiments. Assimilative efficiency is dependent upon the factor of duration. Both assimilating and assimilated elements appear to require more than momentary duration to induce fluctuations in the subjective intensity of the norm.

The Study of Consciousness and the Study of Behavior. EDWARD L. THORNDIKE.

The statements about human nature made by psychologists are of two sorts,—statements about *consciousness*, about the inner life of thought and feeling, the 'self as conscious,' the 'stream of thought'; and statements about *behavior*,—about the life of man that is left unexplained by physics, chemistry, anatomy and physiology, and is roughly compassed for common sense by the terms intellect and character.

The fashionable attitude in psychology has been to restrict its subject-matter to statements about consciousness. But in animal psychology this restriction becomes obviously unnatural and annoying. It appears that in human psychology as well it is unnecessary, and perhaps inadvisable, to continue the pretense that there is an impassable gap—a real discontinuity—between any and all of the animal's movements and his states of consciousness. Scientific judgments about a man's tooth-ache, anxiety, or judgment that 'seven threes equal twenty-one' do not differ thus radically from judgments about his stature or body-temperature. The former differ in their greater variability, greater dependence upon the man's verbal reports, and greater likelihood that one man will have, in respect to them, certain sources of information denied to other men. But the differences are only of degree. The facts of mental science need not be 'known to one observer only' or matters of 'pure experience.' If by the sun is meant the sun of common sense, physics and astronomy, then by a man's pain we can mean the pain of medicine, economics and sociology,—the pain as known to any one, and to the suffering individual long after he *was it* or *had it*. The facts of mental science are spatial. Napoleon's mind lived in France and went to Russia as truly as did his body.

Two noteworthy advantages are secured by the study of behavior. First, the evidence about intellect and character offered by action and the influence of intellect and character upon action are given due attention. Second, the connections of conscious states are studied as well as their composition.

The Relation of Strength of Stimulus to Rate of Learning in the Chick. L. W. Cole.

The experiments were undertaken at the suggestion of Professor R. M. Yerkes in order to learn under what strength of stimulus chicks most rapidly learn to make, respectively, an easy, a medium, and a difficult discrimination. The chicks were taught to escape from the experiment box by choosing always the darker of two passageways, or, rather, by going always toward the darker of two opal flashed glass screens at which the passageways ended.

Three conditions of discrimination were used. For the condition termed 'easy' one screen was illuminated by an electric lamp 33.5 cm. distant, the other screen was not illuminated. For 'medium' discrimination, one lamp was at 23.5 cm., the other at 98.5 cm., and for 'difficult' discrimination the lamps were placed, respectively, at 23.5 cm. and 53.5 cm. from the screens.

For each condition of discrimination groups of chicks of exactly the same age were trained under the influence of a weak, a medium, and a strong electric shock, though even the latter was not of harmful intensity. After necessary preliminary work, the rate of the chicks' learning to make the discriminations for each of the three strengths of stimulus was determined.

When the two screens were easy for the chick to discriminate the number of trials required for perfect discrimination decreased with increase in the strength of the stimulus, *i. e.*, the stronger the stimulus the quicker the learning. For medium difficulty of discrimination this relation did not hold in the case of the strong stimulus. It produced slower instead of more rapid learning. In this particular my results agree with those of Yerkes and Dodson for the mouse. Under the difficult condition of discrimination the chicks divided into two groups, those which learned more quickly the stronger the stimulus, and those which failed to make the discrimination. The latter group included one half the number of chicks tried with the strong stimulus. The chicks which failed made more errors in their early trials and consequently received more pain stimuli at the beginning of the tests than the chicks which succeeded in learning to discriminate the two screens. By determining the threshold of stimulation for the electric current, for six of the chicks, it appeared that the results of the experiments were independent of a difference in sensitiveness of individual chicks.

The behavior of the chicks and the records of their learning seem to indicate that, under the medium condition of discrimina-

tion, the strong stimulus retarded learning in two thirds of the chicks, while, under the difficult condition of discrimination, it completely inhibited learning in one half their number, but in the remainder an increase of stimulus uniformly increased the rapidity of learning.

Some Results in Comparative Psychology. JOHN F. SHEPARD.

It was suggested that the higher forms of control might be divided into four types: (1) Formation of simple connections, a stimulus calling forth the effects of others that have been experienced with it. (2) Trial-and-error, establishing habits out of a mass of hit-and-miss responses. Explained as follows: The neurological unit of attention is the passage of processes over a definite set of synapses changed as in associations. This involves inhibitions. Some combination of hit and-miss responses gives a result having definite significance either on inherited or acquired grounds. This definite result, not pleasure, is the selective factor which builds up the habit. (3) Application of experience to a similar situation, with analysis; made possible when synaptic connections become sufficiently detailed, combined with sufficiently definite inhibitions. Imitation and learning by being-put-through are explained as special forms of this. (4) The plan: A response tending to take place and being excited by another, but inhibited by the incompleteness of that other.

The experiments showed that people learn labyrinths from both ends, although the method of learning is not exactly the same at both.

In the first labyrinth, all rats and cats learned backwards from the food-box first, although some became familiar with the beginning before the region of the second and third errors. In other labyrinths they more often turned correctly when coming out of blinds, they dropped the errors at both ends, and learned the whole in very few trials; but they did not retain the path so perfectly until another day. In going backwards, the animals were most familiar with the end near the old food-box, and learned the end near the new food-box before the middle.

Ants learned labyrinths even when the bottoms of the alleys were renewed repeatedly during learning. They learned less perfectly with light on both sides than with light on one. Rotation of a section produced no effect. Changing the side of the light after learning controlled almost absolutely the movements of the ants. Relearning when the light was changed required three times as long as the original learning. In darkness they were nearly helpless,

possibly retaining a slight motor (?) association. The part near the food-box was followed better in going toward the food-box than away from it; that near the nest, better in going toward the nest. Definite associations were retained at least a week.

JOINT SESSION WITH SECTION L (EDUCATION) OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

Individual Differences in the Correlation of Physical Growth of Elementary and High School Pupils. BIRD T. BALDWIN.

This paper gave a preliminary report of an investigation in the correlations of yearly and half-yearly increments of growth in height and weight of 350 boys and 435 girls from the University of Chicago elementary and high schools, the Francis W. Parker School and the Horace Mann School of Columbia Universi y. The records include consecutive measurements on the same individuals for periods from 3 to 11½ years, giving a sum total of nearly 6,000 measurements.

At the beginning of the investigation a mediam was found for *all* measurements in the two Chicago schools. This mediam shows, among other things, that the children in the three schools form practically a homogeneous group.

Different correlations for growth in height and weight are shown between both boys and girls who are above the median height, and those who are below it. Those above begin and end their various periods of acceleration and retardation, on the average, earlier than those below the median. The present results show the average maximum acceleration in absolute height increment for boys above the median to be between 13 and 14 years of age, for those below, between 14 and 15 years of age; for girls above the median, the acceleration is between 11½ and 12½ years of age, and for those below, 12½ and 13½ years of age. This general conclusion is supported by the average results obtained from all half-yearly increments and all yearly increments, based on the year and half-year, and for the schools individually and collectively.

The present results indicate that the same general law is applicable to weight. The maximum increase in absolute weight is earlier for boys and girls who are above the median height than for those below.

Individuals below or above the median height, as a rule, maintain approximately their relative positions throughout the period

studied, but they vary in some instances in their deviations from the median, and from one another. A few individuals whose measurements lie near the median cross from below to above, or *vice versa*, the smaller number crossing from above to below. A few show no marked changes in relative growth during the adolescent periods.

The fifty-two individual curves for both height and weight support the general conclusions stated above, and also indicate how one may prophesy the probable development of a boy or girl after once knowing his or her relative position in reference to the given median. They also demonstrate that the correlations in weight do not follow in detail the correlations in height, since there are more fluctuations in acceleration and retardation, and in several instances an actual loss in weight.

Experiments on the Perception of Number in Children and Adults. FRANK N. FREEMAN.

The experiments here reported had for their purpose the comparison of the scope of attention and number perception with visual objects in adults and children from 6 to 14 years. The objects were spots of light 1 cm. in diameter and the same distance or more apart. They were presented tachistoscopically upon a large cotton screen, the source of light being hidden by the screen.

For the scope of attention the objects were arranged at equal intervals in a horizontal row. Any other arrangement favored subjective grouping. The perception of number was also studied with objects grouped in various ways.

The percentage of correct judgments with an increasing number of objects decreased more rapidly in children than adults. This probably indicates, first, a better command by adults of grouped as compared with ungrouped objects. Not only do adults apprehend objects which are objectively grouped better than children but they have an advantage in dealing with objectively ungrouped objects which are beyond the scope of attention by means of subjective grouping.

The chief difference is not, then, in the scope of attention. This was estimated, with the aid of introspection, to vary in adults from between 3 and 4 to 6. The median was between 4 and 5. The judgments of children of 8 to 10 years averaged only 4.5 per cent. poorer than those of adults up to 4 objects. It appears then that the difference in scope of attention is less than 1. Beyond 4 objects, however, the difference in correctness is much greater—for 5 being

22 per cent. This is attributed to subjective grouping on the part of adults. The fact that there is little difference between horizontal ungrouped objects and objects grouped by threes is evidence of this.

A coöperating factor in the greater correctness of judgments of adults is the superior distribution of attention, which is especially prominent in the judgment of a complex group in which one of the sub-groups is different in number from the others. Children first attain proficiency in recognizing horizontal group forms.

Pedagogically it would seem of value to introduce number work by training in the recognition and manipulation of grouped objects. This method could well be used, as in Germany, for drill in the fundamental number operations.

The Genesis of Attention in the Educative Process. EDGAR JAMES SWIFT.

Attention means a certain arrangement of the content of consciousness, which gives clearness to one idea or group of ideas, and produces comparative, though not equal, obscurity of the others. Change of attention requires a redistribution of the content, and this is accompanied by a rearrangement of clearness.

The educational problem is to secure attention to certain ideas which usually lack the attractions needed for success in the competition with diverting ideas. The feelings cannot accomplish this because hateful ideas may be as attractive as those which are pleasant. Besides, the emotional attitude toward an idea cannot precede attention to it.

Rewards and punishments fail; the first because of uncertainty regarding the sort of knowledge that will secure the reward, and the second because it makes the implied penalty and the teacher too prominent in the consciousness of the learner. Further, both of these incentives divide the attention.

In boys, attention to ideas must be secured by identifying these ideas with the racial instincts. The ideas and activities of children are the stuff out of which their thoughts are made. In early life this material is social, and it is social because it is racial. The force of this social instinct is seen, among other ways, in the number of clubs formed by boys without the assistance of adults.

Children are rarely inattentive to work which they regard as their own. The group sentiment is always active in determining what ideas shall occupy the focus of consciousness. Making children feel that the work is theirs, and not the teacher's, means securing

attention. This can only be done by utilizing the racial instincts in the educative process.

The productive efficiency of the energy released by group sentiment is seen in the results accomplished under the name of play. It is not the nature of the activity that distinguishes work from play so much as the mental attitude assumed toward the occupation.

The utilization of the racial instincts in securing attention to educative ideas has been resisted by schoolmen largely because of the educational dogma of effort. Attention does its best work when the feeling of effort is wanting. Effort indicates resistance or strain, and accompanies inefficient attention.

Attention is determined by past and present states of consciousness. In childhood, these states of consciousness are largely racial and social, and continued attention can only be secured by creating educational situations in which the school consciousness loses its identity in the racial and social consciousness.

(*a*) *Periods of Work in Learning.* D. STARCH.

The purpose of this experiment was to determine the relation of the length and distribution of periods of work to economy in learning. The learning consisted in associating numbers with letters. These associations were formed while transcribing prose into numbers.

One group of persons worked 10 minutes at a time twice a day for six days. The second group worked 20 minutes at a time once a day for six days. The third group worked 40 minutes at a time every other day for six days.

The records show that the 10-minute group improved more rapidly than the 20-minute group and the latter improved much more rapidly than the 40-minute group. The 20-minute group transcribed on the average 31 more letters in every five minutes than the 40-minute group and the 10-minute group transcribed on the average ten more letters in every five minutes than the 20-minute group.

(*b*) *Transference of Practice.* D. STARCH.

The object was to determine the effect of training in one fundamental arithmetical operation upon the efficiency in other arithmetical operations.

A group of persons was trained for fourteen days in mental multiplication. Before and after this training they were tested in other arithmetical processes, such as adding fractions, adding, subtracting, and dividing numbers. These tests were also made upon another group of person who did not do the fourteen days of training. The

improvement which this group of persons made was deducted from the improvement made by the first group. This left an improvement 56 per cent. as large as the improvement in the mental calculation during the fourteen days. This improvement was due almost entirely to the ability, acquired in the training, of holding the numbers better in mind.

The Consulting Psychologist. C. E. SEASHORE.

The consulting psychologist is an expert in psychology who may be employed as adviser in matters pertaining to the ascertained facts of mental life with reference to their bearing upon a given practical situation, or may be employed to search for, or verify, such facts by special investigation. This field of work is not covered by academic teaching or research, nor by the common application of psychology in practice. The work of the consulting psychologist falls roughly within the four general fields: (1) mental pathology, (2) education, (3) the arts, crafts, professions, and industries, and (4) eugenics. The scope of these fields is illustrated in detail and a concrete illustration of actual work is given. The consulting psychologist must be thoroughly trained in laboratory psychology and be fully conversant with the practical field in which he is to act, and must rise through a series of apprenticeships. Some of his essential traits and limitations are illustrated negatively: he is not an administrative officer, not a practicing physician or teacher, not a pure scientist, not a reformer, not deprived of scientific freedom. He has chosen some phase of applied psychology as his profession and pursues it intensively as a life work.

JOINT SESSION WITH THE WESTERN PHILOSOPHICAL ASSOCIATION

Topic: Philosophical and Psychological Usages of the Terms Mind, Consciousness, and Soul.

J. R. ANGELL.

The term soul has generally been applied to the supposed spiritual essence of human personality which persists after death. As such it is connected with problems not soluble by ordinary empirical methods. Psychology as an empirical natural science has consequently ceased to use it as a familiar part of its terminology.

The term mind as meaning a durable psychic entity has also come to enjoy a highly precarious position. William James' defense

of the 'Thought of the Moment' as the only thinker needed in psychology is the classic expression of the passing of the old-fashioned conception of the mind. Mind as a term applicable to the entirety of mental phenomena, but not to a stable entity, continues to serve a useful function.

If concrete psychological events could be explained mo e effectively than otherwise by the hypothesis of a soul or a permanent mind, no doubt these terms and their corresponding concepts would still be actively represented in our literature. But this is not generally thought to be the case.

Signs are not wanting that the term consciousness itself is likewise in dange of extinction or at least essential modification. As a class name valuable for designating a group of phenomena presenting peculiar problems, it will presumably long remain with us. This will no doubt prove true despite the difficulty of defining it and despite the efforts of certain metaphysicians to reduce consciousness to one among other relations sustained to one another by objects. But there is unquestionably a widespread movement on foot in which interest is centered in the *results* of conscious process, rather than in the *processes* themselves. This is peculiarly true in animal psychology; it is only less true in human psychology. In these cases interest is in what may for lack of a better term be called 'behavior'; and the analysis of consciousness is primarily justified by the light it throws on behavior, rather than *vice versa*.

If this movement should go forward, we should probably have a general science of behavior, recognizing two main sub divisions, physiological and psychical.

In any event this is a period in which sharp distinctions of one science from another are commonly regarded as both impracticable and unprofitable. With the movement in psychology over toward biological and physiological conceptions, it may reasonably be expected that the word consciousness will take on more marked dynamic and functional characteristics, so that even if the term persists, it will undergo material alteration in its implications.

B. H. Bode.

The realistic movement has contributed a variety of definitions of consciousness, the definitions here discussed being those which identify consciousness respectively with awareness or apprehension, with context, or setting, and with the function of representation or meaning. The first of these is obliged to recognize two kinds of

objects, viz., those which exist only when there is awareness—*e. g.*, pleasures and pains—and those which may exist apart from awareness. Hence it is obliged to postulate two different types of response on the part of the perceiving organism, the one being a condition of awareness only, while the other is a condition both of awareness and of those qualities or objects which exist only when there is awareness. This implication of the position finds no support in the present-day psychology of perception. The actual test between the kinds of objects is not this hypothetical difference in our responses, but in context or relationship. In applying this test, however, the realist confuses the distinction between fact and meaning or validity with the distinction between valid fact and validating experience. This same confusion occurs in the form of realism which discards awareness and finds in context or relationship the source of the differentiation between consciousness and object. It, therefore, has no advantage over the other theory. The assertion that consciousness is the function of meaning assumes a hard and fast distinction between sense-quality and meaning, which does not exist in fact. The view of instrumentalism, which has much to say for itself, is that consciousness is a name for the entities created by the psychologist in the furtherance of his particular purposes. It is not a distinct entity or function or relationship. The business of the psychologist is with the acts which constitute the course of experiencing.

G. A. Tawney.

The ancients were familiar with the facts that man may know his own knowing and that many men know the same things, con-sciousness. But they had no terms which regularly denoted these facts, and they knew no problem of consciousness such as ours. They were interested in the relation of the universal to the particular, of form to matter, both terms of the problem being for them aspects of existence. Christianity *et al.* laid emphasis on the soul and the heart with their motives, sins and destiny. Like some of the ancients, it identified these with the man. Descartes distinguished thinking substance from extended substance, conceived minds as individual things related to each other and to other things, and used the new term consciousness to mean the mind's recognition of its own contents. The word consciousness already meant the knowledge by many minds of the same things. Descartes was chiefly responsible for the doctrine of the subjectivity of the sense-properties of things and for the distinction between the primary and secondary properties.

Locke, and the early English psychologists generally, use consciousness to mean the mind's perception of its own processes. But they also tend to use it for these processes themselves. The empirical aspect of mind is identical with consciousness. Metaphysically, mind is something more than matter in motion. Thus, mind or consciousness is the *locus* of ideas, emotions, choices, etc., and is made up of these things themselves. Most of the central problems of modern philosophy grow out of this conception. Romanticism and especially the romantic philosophers, such as (*e. g.*) Kant, emphasized the independence and autonomy of 'inner life' or 'inner world,' and modern psychology is, on the whole, the science of this 'inner life,' its relations to the body and the 'external' world, its elements and the laws of their compounding, etc. The terms 'mental states,' 'states of consciousness,' etc., do not alter the situation. The functional psychology defines function in terms of reflex sensorimotor process,—an adjustment of the traditional conception of consciousness to the laws of biology and physiology. It still discusses the relation of mind to body and other problems growing out of the traditional conception of consciousness. It fails to lay a scientific foundation for logic, ethics, the philosophy of religions and sociology, which deal with actual human experience and human beings rather than with that ideal construct called mind or consciousness. What is needed is a psychology of that kind of behavior which is marked by immediate value, selective repetition and, to use words of much narrower connotation, intelligence and character. If we could return to the objectivity of the ancients it would be a distinct gain. Of recent attempts to revise the traditional conception of consciousness, the paper gave special attention to Woodbridge's definition of consciousness as an order of relations.

E. H. Lindley.

The recent developments in the domains of animal behavior and of abnormal psychology have subjected the orthodox conceptions of consciousness to a severe stress.

The present trend toward a purely objective study of behavior (Thorndike, Judd) tends to eliminate consciousness and to substitute experience or intelligence, terms as yet equally vague.

The problem of the subconscious, often conceived crassly enough, sets a real problem. The hypothesis of unconscious cerebration, on the other hand, carries a load already staggering. And in as much as we know less about the brain than about consciousness, the effort

to banish all the difficulties of psychology to the limbo of unconsciousness, seems little short of evasion.

Awareness as the criterion of consciousness is attacked from two radically different quarters. Dr. Morton Prince has shown that in a case of dual personality a somewhat elaborate computation was carried on without the awareness of the dominant personality of the moment. And Professor Titchener in his *Experimental Study of the Thought Processes* says: "I doubt if meaning need be necessarily conscious at all." And he describes a given recognition as "simply a recognition without consciousness." He and other writers insist that the *Aufgabe*, which determines the trend of consciousness in the solution of problems, may be unconscious. In short, the most essential factors in the composition of so-called conscious attitudes are themselves unconscious. In a word consciousness and awareness are not coextensive. These cases from normal and from abnormal psychology emphasize the dilemma in which our science finds itself.

If psychology minimizes the conscious factor, if it subordinates the pyschic to the physiological, does this not involve the surrender of the peculiar and unique task of psychology as distinguished from biology and physiology? Is it not precisely the problem of psychology to make the most of consciousness rather than to minimize it?

To conceive consciousness in terms of levels or forms and not as a point; to employ to the utmost the principle of gradation, so fruitful elsewhere; to examine exhaustively the evidence for the subconscious; to try out the hypothesis that the transition from neural to conscious is an elaborate series; to be stimulated by the new conceptions of energy, now developing in physics; to hope for a new and illuminating statement of the psychophysical relation; and to seek to conserve to the conscious end of the series all the distinguishing marks that truly belong thereto; to supply for the psychology of religion and for abnormal psychology a theory of consciousness adequate to their needs; such constitute a portion of the urgent tasks of psychology.

Case-taking in the Psychology of Religious Experience. ROWLAND HAYNES.

By case-taking is meant a procedure similar to that of the physician or psychiatrist. It uses as sources of information observation of the subject under questioning and tests as in a clinic, the answers of the subject studied, and the testimony of friends and relatives where obtainable. It includes information on the family history of the

subject, his past religious history, and his present religious status. On the basis of this information it seeks to diagnose this individual's religious experience, to prescribe treatment whereby the individual may make the most of his own religious capacities, and to observe the subject's reaction under treatment.

Studying the method used in the psychology of religious experience we find first, the genetic method, much in vogue at present. This is of value in giving a proper background for all study of religious experience. Its practical usefulness is limited by the fact that it covers the geology rather than the chemistry of religious experience. Second, the biographical method is case-taking at long distance and is handicapped by usually having only snap-shots of striking events in the religious careers of extraordinary people. Third, the questionnaire gives occasional helpful suggestions, but for careful diagnosis is about as valuable as letters answering patent medicine advertisements, since the patient frequently doesn't know how to describe his case accurately without questions from the interviewer adapted to his particular case. Fourth, laboratory tests, which reveal past experiences of the subject, are to be distinguished from laboratory experiments, which manipulate experience in the laboratory. Such tests are of great value when used in connection with case-taking and the past history of the subject.

Case-taking in religious experience has two difficulties. One, that of having the clinical picture distorted by the notions of the case-taker, it shares with case-taking psychiatry. The other is peculiar to the psychology of religious experience. In psychiatry the facts of abnormality are easy to note. In religious experience the relevance of facts to be noted is not always easy to determine.

A sample case was given to illustrate the method.

The Question of an Ultimate Religious Element in Human Nature. Irving King.

Many thinkers, both religious and scientific, have tended to postulate an ultimate religious element or instinct. The religious thinker does this primarily because he feels that a natural origin of religion would rob it of all its value. The scientist, on the other hand, who traces religion to a primal instinct probably feels that the apparent universality and persistence of religion point to such an origin. He may also be led to this view by too narrow a construction of the axiom that every effect must have an adequate cause. From this latter, he assumes that the religious quality can have come only

from something which was also religious. It was pointed out in the paper that none of these grounds are sufficient for assuming a religious instinct. Neither is a regulative instinct a plausible hypothesis. It is not clearly evident that the primal instincts of living beings need a *deus ex machina* in the form of a regulative instinct to bring them into harmony. Instincts decidedly out of harmony with the welfare of the organism or of the race could hardly develop.

It seems much simpler and closer to the facts of human nature to think of religious attitudes as 'constructs' dependent upon the social environment for their appearance in each individual and in each generation. The large non-religious classes of today are probably genuinely irreligious and testify to the need of a favoring social environment for the production of the religious attitudes.

The Instinctive Bases of Religion. EDWIN D. STARBUCK.

Religion must surely be regarded as one among the human instincts. In a genetic approach there are at least three ways of accounting for the instinctive nature of religion. The first is to show that there is a particular quality of the mental life traceable throughout its evolution, of which the religious instinct is the developed equivalent. This paper will chiefly be concerned with this contention. The second way for accounting for it is in terms of an acquired sense through experiences of certain types of useful social adjustment. The third method is to call religion a fusion or compound of the various normal human instincts.

The limitation of this point of view is in explaining how the harmony among the various instinct-ingredients in the religious consciousness has been preserved. There must be a delicate sense of proportion or relation or fitness or harmony that directs consciousness and determines at each point the particular advantageous response or emphasis. Such a sense among animals has been admitted by genetic psychologists generally. There are two phases of this. The first is the direct and immediate evaluation of the worth of a reaction to the organism. This I propose to call the *cosmo-æsthetic* sense, following the original meaning of the word kosmos—order, relation, proportion. Without this I believe it impossible to understand the non-rational successes of religion, art and morality in feeling their way into higher modes of reaction and into a profounder sense of reality. But the cosmo-æsthetic sense alone could give only a '*retrospective*' evaluation. There is, however, the 'prospective reference' as well. I propose to call this the *teleo-æsthetic* sense. The religious impulse reaches out

toward an ideal fulfillment. Vital organisms also seem to act under the influence of an obsession toward self-enlargement and a feeling after that type of reaction that will complete and fulfill the present need. On such an assumption one can describe consistently by the methods of genetic psychology the anticipatory qualities in moral intuition, the non-rational behavior of the poetic passion and the apparent success of religion in feeling after an adjustment or an ideal reality. I venture to hope also that the recognition of a cosmo-æsthetic and teleo-æsthetic sense will bring a body of useful description to the field of the subliminal consciousness which has been a too convenient dumping ground for the ugly problems of the psychology of religion.

The Genesis of the Group Spirit. EDWARD S. AMES.

Any company of persons coöperating together in a common enterprise develop a sense of unity and a feeling of deference toward the combined judgment and authority of the group. This is experienced in slight degree by persons brought together in such casual relations as those of fellow travellers in a railway coach. In more sustained associations, such as workmen in a factory experience, there is built up a more elaborate and persistent group feeling.

What psychological account is possible of this sense of the social whole? Imitation is not a sufficient explanation. The experience is too complex to be stated in terms of the relation of the ego and alter, or that of the ego and many others. Not only are the members of the group in interaction among themselves, but they are also reacting in reference to the objects and plans which go beyond the immediate situation. This complexity may be described still further. The ego itself is a more or less closely organized community of our various social, material and reminiscent selves. It involves, therefore, relations of the most organic sort with other similarly complex egos, with which it forms what we call the group. Within this group there are many ego and alter relations, but this group itself possesses its group organization with reference to some alter or to some plan of action which is objectified by the group. Thus the members of a given family stand in reciprocal relations, which reciprocal relations are determined in turn by reference to the neighborhood life, to other families, and to the plan and sphere of action characteristic of the family. The consciousness of the family group arises not through the imitation and opposition of the members, but through their coöperation in reaching certain ends. The sense of the group

comes to consciousness with the effort to work out this coöperation. This coöperation is carried on through stimulation and response, the response being not always an imitation of the stimulating act, but more often a different reaction for which the stimulation is the cue. In most of our social experiences the responses called forth by our acts are of a different kind yet organically related to the attainment or hindrance of a common end. Our acts, or attitudes, that is, our tendency to action as expressed in the truncated acts we call gestures, become in this way the stimuli to corresponding acts or attitudes on the part of others. All communication, and especially all face-to-face communication, has therefore this most stimulating and vivid quality of the nascent reproduction of complex situations, of coöperation and conflict with reference to the further conduct of all the social groups represented. In this interplay of conversation there rises a complex background of associations and implications which is felt by the participants as something urgent and insistent, suffused with the warmth and intimacy of a personal quality.

The Several Origins of the God-Ideas. JAMES H. LEUBA.

In this paper psychological and historical arguments are advanced in support of the following propositions:

1. Gods grew out of several different ideas of superhuman, unseen, personal beings.

2. These beings had an independent origin.

3. The attributes of the gods differ according to their origin.

4. The historical gods are usually mongrel gods, the outcome of a combination of characteristics belonging to beings of different origins.

In a second part, applications are made of principles resulting from these propositions:

(1) To the solution of the difficulty produced by the simultaneous presence among primitive societies of apparently irreconcilable facts, namely, the presence, even among the most primitive tribes now living, of a Great God high above the others, to whom is usually assigned the function of creator; the existence among these same tribes of a rabble of spirits and ghosts; and the increasing influence and artificiality of sacrifices and prayer "in proportion as the idea of a Supreme Being grows dim."

(2) To the detection of the confusion of the impersonal Absolute, a god of metaphysical origin, with the Christian God, an empirical, personal one. It is this confusion which leads to utterances such as

this, "psychology neither rejects nor affirms the transcendent existence of the religious object; it simply ignores that problem as being outside of its sphere."

The failure of anthropologists and of historians to recognize several sources of superhuman, unseen agents and the distinct gods arising from them, is held to be one of the chief causes of the unsatisfactory condition of our knowledge regarding the beginnings of religion.

An Unusual Case of Color-blindness. FLORENCE RICHARDSON.

The subject, a young man about 24 years old, first showed difficulty in getting certain colored negative after-images. Tested with the Holmgren wools he matched the red skein normally; the matches for green were few, but correct; but for the purple he chose pinks and light reds, asserting that the red and purple skeins were of the same color, but that the purple was a *thin* red, while the other was a *good* red, and so far as mere color was concerned, any skein that would match one would match the other. A skein approximating a standard blue he matched hesitatingly with light yellows, light greens, very light browns, light grays and other blues. Given a yellow skein, he matched it with the darker of the light greens, browns, grays, blues and yellows, stating that the blue and yellow skeins were alike except as to brightness, the blue being much brighter than the other and neither of them having color.

The subject has never had any serious illness. An ophthalmological examination shows no pathological condition of the retina. The visual acuity is normal, and there is no coloration of the lens. There are color defects in the immediate ancestry. Everything favors the assumption that this is a congenital and not a pathological case.

Tests with the Nagel cards and the Hering tester showed an inability to discriminate yellows, blues and grays, a normal sense for red and a weak sense for green.

A spectroscopic examination showed that his spectrum is not at all shortened. A first neutral zone begins at the *D* lines and continues to the *F* line where the green appears for a *very* narrow band, and is often not present. A second neutral zone extends from this point almost to the *H* line where the red begins again. This violet end of the spectrum he refers to as the thin red end. The point of maximum brightness is in the blue.

Of the Hering colored paper discs, the orange, yellow, blue-green, green-blue, and blue are selected as colorless.

The subject obtains red negative after-images with ease, even when the original stimulus is not green to him, but gray. Only in a very faint illumination are colored after-images from red obtainable. The after-images of blue and of yellow, naturally uncolored, differ only in brightness. He has not, so far, been able to obtain positive after-images. He cannot fixate a strongly illuminated point.

The black-white retinal zone is approximately normal. Naturally the blue and yellow zones are not present. The red zone extends irregularly from 30° on the temporal meridian through 30° on the upper and lower vertical meridians and out to 60° on the nasal meridian. The green field in much constricted, though at one point it overlaps the red.

A Case of Colored Gustation. JUNE E. DOWNEY.

The paper reported a detailed analysis of a case of colored gustation. Evidence was presented that the sense of taste of the young man under consideration is defective, the evidence being particularly strong for the taste of bitter. Through reliance upon the tactual and color components of the taste-blend, the young man was, however, enabled to pass accurate taste-judgments. His threshold for the presence and the recognition of the four standard tastes was found to be average or, in the case of bitter and sweet, slightly better than average. Extensive stimulation and the presence of odor increased the vividness and persistence of taste-colors. These colors did not appear in minute stimulation of the tongue.

The taste-colors were definitely localized in the mouth, were of hallucinatory vividness, and had a uniform color-tone and persistence under constant conditions. The presence of objective colors introduced noticeable conflicts with the taste-colors. The color values for the different tastes were as follows: bitter, a dull orange-red; sweet, a brilliant black; sour, red or brown and, at times, possibly green; salt, if strong, induced a crystal-clear experience, if weak, gave a slate or gray color. The salt experience was found to neutralize or rather to banish the color of sour, and to partially neutralize that of sweet. The colors of sweet and sour were found to fuse; those of sweet and bitter to conflict.

For several flavors an alternation of green with a violet-pink taste was reported with great constancy. Possibly this alternation could be explained as that of a negative after-image. Other shifts from one color to its complementary were reported infrequently.

It was found that the presence of a vivid taste-color noticeably retarded the recognition even of very strong flavors.

The interpretation offered for the case is that the synæsthetic element is a true component of the perceptual fusion and in the present instance easily accounted for by the color stimulation present in taste-experiences, a component which has in this case acquired an unusual value through a lowered sensitivity of taste. In general it is suggested that the motor attitudes common to different forms of sense stimulation may explain many cases of synæsthesia, particularly if there be an inherited tendency to dissociation or if such a tendency be induced by disease or by abnormal conditions of the sense-organs.

A Useful Demonstration of Tonal Fusion. W. VAN DYKE BINGHAM.

Following upon the usual analytical demonstration that the sound given out by the string of a monochord consists of a fundamental tone and a series of upper partials, a synthetic procedure is effective. The clang may be reconstructed by the fusion of the pure tones given out by a series of forks having the pitch of the partial tones which have been found to be the components of that clang.

Wundt makes the statement that this is impossible, because it is necessary that there shall be only one predominating element in the fusion; and lest more than one tone be prominent, "all the partial tones must be sounded at exactly the same time." This difficulty can be met in the following manner. The reeds of a harmonium are tuned to the exact pitch of the forks, which are mounted on resonance boxes. By means of a depression bar one plays for some seconds a loud chord which includes among its notes all the pitches of the desired partial tones. Thus the several forks are gradually set into sympathetic vibration. If now the loudly sounding harmonium is suddenly silenced, there is heard a simple clang, strikingly resembling in color the sound of the monochord. Indeed, the listener is at first apt to judge that the monochord string has somehow been actuated.

The forks used by the writer in the present demonstration are the Koenig series, $c', g', c'', e'', g'', 7b$ flat$''$, c''', d''', e'''. It will be noted that these are the upper partials of the fundamental, $c = 128$ v.d. Although no fork of 128 v.d. was used, the clang as heard seemed to have that pitch. The explanation offered is that since each of the forks makes with its next neighbor in the series a subjective difference tone of 128 v.d., the total effect is a difference tone sufficiently intense to strongly predominate over the weaker objective higher tones and to serve as fundamental for the clang.

The Adjustment of an Asymmetrically Distributed Series of Contradictory Judgments. HERBERT WOODROW.

The sort of series here considered is that obtained by the method of minimal changes, in those cases in which there occurs a 'doubtful zone' including an unequal number of 'greater' and 'not greater' judgments. Series in which the doubtful zone contains more 'greater' than 'not greater' judgments (in the case of the upper differential limen) are often obtained, and are to be expected, with especial frequency whenever the distribution of limina is asymmetrical. In such cases the method of taking the average of the limits of the doubtful zone is unjustifiable. This fact has even caused some investigators to discard the method of minimal changes. A satisfactory value may, however, be obtained from such series, in a perfectly logical manner. The presence of inversions in the series means that the limen has not remained constant even during the time required to go through one series. But one may find out what the limen is on the average for all the instants at which judgments were given. This can most easily be done by analyzing the series, which contains within its doubtful zone asymmetrically distributed 'greater' and 'not greater' judgments, into a set of component series, all of which component series are made up of equal-sized steps, and present no inversions. It is always possible thus to analyze an asymmetrical series into such a set of symmetrical series. This set of component series contains all the data contained in the original series and assumes nothing not found in the original series. In the case of each of these component series it is justifiable to take the usual average of the quantities giving the last 'not greater' and the first 'greater' judgments as the limen for that series. The average of the limina obtained from the component series gives the average limen for the original series. It is a quantity to which attaches the probability $\frac{1}{2}$ for a 'greater' judgment. This method takes into account every 'greater' and 'not greater' judgment contained in the doubtful zone. Whenever the distribution of these judgments within the doubtful zone is asymmetrical, the result obtained by the method described will differ from the middle point of the doubtful zone. In practice it is unnecessary to resolve the original series into components, and to take the average of the limina derived from them. The same result will always be obtained simply by transposing the 'greater' and 'not greater' judgments of the original series from the quantities at which they occur to other quantities represented in the series, until all inversions are gotten rid of, and

then taking the point midway between the last 'not greater' and the first 'greater.' This procedure requires the size of the steps of the original series to be equal, but does not require limitation of the subject's judgments to two categories.

Two Simple Test for Fatigue, with Some Records. GERTRUDE KLINE AND J. B. MINER. (Read by title.)

The attempt was made to devise a home experiment for motor and another for mental fatigue such that the change in endurance and efficiency could both be measured. The motor test was a combination of the tapping and strength tests attained by a regulated movement of the index finger between the table and a horizontal cardboard surface. The mental test was a modification of the computation test in the form of continuous division examples in which each division by 6, 7, 8 or 9 was carried three places and thus provided a new dividend. In each test there were four series of 25 examples or movements each, with ten seconds interval between each series allowed for making the record in seconds. Endurance was measured by the ratio of the fourth to the first series. The experiments were tested for nine periods during the day on one subject for 27 days and on a hundred college students for one day. The results indicated that an hour's continuous practice would be enough to avoid the disturbing effect of practice, and that, with this eliminated, the motor test was exhausting enough to determine individual diurnal curves for both efficiency and endurance, while the division test was not sufficiently severe for college students. Experience with the motor test also indicated that the height of the cardboard surface might better be defined by the length of the middle finger and the subject required to keep only his thumb, third and fourth fingers and forearm in contact with the table. Before each test estimates of the degree of weariness and of the amount of physical and of mental activity immediately preceding the period were made by the subjects on a scale of five. These were used to interpret the fatigue curves. The Chauvenet formula for rejecting extreme cases was advocated as a means of indicating their effect on the curves in experiments where conditions are not all under laboratory control. In these experiments such rejection changed the form of the group curves but slightly. Home tests on a large group offer an important check on laboratory tests of a few subjects. Tests like those described when repeated at intervals show what people naturally do, while a test given but once or twice shows what they

can do when stimulated to it. Before the final form of the diurnal curve for these tests is determined the results given should be checked by the same experiments on a smaller group under laboratory conditions.

The Size-distance Problem and its Experimental Solution. JOSEPH JASTROW. (Read by title.)

The size-distance problem is typical of the physiological support of the perspective processes. The common view emphasizes that we bring into play the physiological mechanism, and upon the basis of the data thus contributed build up a psychological interpretation. Conforming to this view is the natural supposition that the size-distance processes act similarly under varying conditions, and that their contribution when separated from the psychological setting, likewise reveals the typical procedure in ordinary vision. The view maintained in opposition to this is that the real significance gives a reverse order of values to these factors; it maintains that as a fact, the psychological processes lead, awaiting only the slight suggestions of a physiological situation; furthermore, that the mind is keenly anxious to reach an estimate by clearing up the factors of perception and to shirk or ignore physiological processes as much as possible. Accordingly their independent status would show a very meager power to solve such problems as the size-distance relation. A preliminary series of experiments with an apparatus devised to produce points of light in a darkened room under controllable conditions seems to support this position. It shows that estimates as well as settings of distance under those circumstances are extremely vague and variable, and that even here the eyes will seize upon any factor, such as brightness of the light to avoid and evade the basis of the size-distance estimate. The experimental attack upon the problem proposes further to add one by one the possibilities of extraneous inference and interpretation—such as a slight amount of light in the room, the presence of standard objects for comparison, the estimate of distance when walls and floors are visible and when not—and all of these in a preliminary fashion corroborate the position here taken. If these conclusions hold, the entire conception of the physiological support of the visual processes must be entirely revised and the precautions of an experimental procedure made indefinitely more rigid, for they show that the slightest suggestion even though subconsciously availed of, is sufficient to transform a seemingly physiological judgment into an extraneous psy-

chological inference. Stated crudely, we seem disposed to use any and every method to guess at distance by clues and signs rather than to judge them through the recognized physiological processes. The latter are doubtless real and in certain situations efficient, but their practical limitations seem thus established. The contribution is presented at once for its experimental and its theoretical interest.

An Experimental Study of the Fusion of Brightness and Color Sensations. The Locus of the Action. C. E. FERREE. (Read by title.)

Studies in the Psychology of the Feebleminded. EDMUND B. HUEY.

American institutions for the feebleminded are establishing departments of psychology, and these are using the classification and terminology tentatively adopted by the American Association for the Study of the Feebleminded, at its recent meeting at Lincoln, Ill. In brief, "feebleminded" is to be "used generically to include all degrees of mental defect due to arrested or imperfect mental development as result of which the person so affected is incapable of competing on equal terms with his normal fellows or managing himself or his affairs with ordinary prudence." The feebleminded are of three classes: 1. Idiots—"so defective that the natural development never exceeds that of a normal child of about two years." 2. Imbeciles, above two but not above seven years of mental age. 3. Morons, above seven but not above twelve years of mental age.

There are low, middle and high grades of each, and epileptic, mongolian, etc., are used as qualifying adjectives. The Binet scale has been very useful in determining mental age, but additional scales and norms are needed. Children not retarded more than three years (two years if not above nine) may be recorded as 'retarded,' using backward as the popular equivalent.

Examination and classification in these terms, at the Illinois Institution, shows that of a year's consecutive admissions imbeciles are most numerous, then morons, with fewest idiots, though a mental age of one and one half to two years is the most frequent of all. Admissions above the mental age of ten are infrequent and hard to retain. Above a mental age of twelve they are almost unknown, though this is the most populous and most troublesome zone of defect.

Special study of 32 of these border cases shows the lines of transition from feeblemindedness to 'normal' dulness and instability, to neurasthenia, hysteria, insanity, and epilepsy and suggests these and other groupings of border cases. The characteristics of each group should be brought out by further clinical studies.

New tests are needed in various directions and university departments of psychology and education might well coöperate with the clinical laboratories in dealing with certain insistent clinical problems, as (1) the selection of the mental functionings which best represent the individual; (2) the selecting and devising of tests which are real tests of these functions; (3) application of these tests to sufficient numbers of successive ages of normal children, to establish norms for all the important functions.

PROCEEDINGS OF THE SIXTH ANNUAL MEETING OF THE SOUTHERN SOCIETY FOR PHILOSOPHY AND PSYCHOLOGY, CHATTANOOGA, TENN., DECEMBER 27 AND 28, 1910

Report of the Secretary

The Sixth Annual Meeting of the Southern Society for Philosophy and Psychology was held at Chattanooga, Tenn., on Tuesday and Wednesday, December 27 and 28, 1910, in conjunction with the annual meetings of the Southern Educational Association. There were, in all, four sessions. The meeting was opened on Tuesday forenoon, Professor Edward Franklin Buchner presiding, in the Assembly Room of the Jewish Temple. On Tuesday afternoon occurred a joint session with the Child Study Department of the Southern Educational Association which was held in the Unitarian Church and, in the absence of the president of the Child Study section, was presided over by Professor David Spence Hill, the secretary of that department. Following this joint session, came the address of the president on 'Learning and Forgetting.' The Wednesday sessions were held at the Jewish Temple, the Child Study Department being invited to join its program with that of the Society in the afternoon session. With one exception, the program was carried out as announced, and the various papers were well received and vigorously discussed.

At the business meeting the officers for 1911 were elected as follows: *President*, Dr. Shepherd Ivory Franz, Government Hospital for the Insane, Washington, D. C.; *Vice-President*, Professor A. Caswell Ellis, University of Texas; *Secretary-Treasurer*, Professor Robert Morris Ogden, University of Tennessee. Elections to vacancies in the *Council* constituted the elective membership of that body as follows: to serve one year, Professor David Spence Hill, Peabody College for Teachers, and Professor W. C. Ruediger, George Washington University; to serve two years, Professor Bruce R. Payne, University of Virginia, and President Haywood J. Pearce, Brenau College; to serve three years, Professor Edward Franklin Buchner, Johns Hopkins University, and Professor William Benjamin Smith, Tulane University.

The report of the treasurer, presented and approved by the Council, showed a balance on hand of $77.21. It was voted that the expenses of the secretary in connection with this meeting, to the extent of $15.00, be borne by the Society.

On nomination of the Council, the following were elected to membership in the Society: Professor Bird T. Baldwin, University of Texas; Dr. Robert S. Carroll, Asheville, N. C., Rev. Frank Philip Hiner, Walnut, N. C.; Mr. H. M. Johnson, Johns Hopkins University; Miss Grace Helen Kent, Government Hospital for the Insane, Washington, D. C.; Professor Thomas A. Lewis, Richmond College; Professor Arthur O. Lovejoy, Johns Hopkins University; Dr. John M. Moore, Nashville, Tenn.; President Robert P. Pell, Converse College; Professor Jno. Pickett Turner, Vanderbilt University, and Mr. George R. Wells, Johns Hopkins University.

It was voted that the next meeting of the Society be held in Washington, D. C., in affiliation with the American Association for the Advancement of Science, provided that the American Psychological Association and American Philosophical Association take like action.

A resolution of thanks was voted to Rabbi Miller and the congregation of the Jewish Temple for their hospitality in providing the Society with a convenient place of meeting.

Abstracts of Papers

Some Supposed Racial Tendencies of the American Negro: A Plea for a Psychological Study of the Race Problem. Thomas P. Bailey.

The race problem is primarily one for the practical psychologist. Prejudice, pride, enmity; the possibilities as to the two races living together; the kind of education needed by the Negro;—all these are psychological problems. Popular psychology has decided that the average Negro is strong in such sensational tendencies of character as the appropriative, the gregarious and the expressive; and weak in such relational tendencies as the assertive, the reponsive and the perceptive (observational). The Negro is regarded as sensational and affective rather than intellectual and volitional. His imagination is thought to be merely concrete and sensuous, his assimilative powers as superficially analogical, his imitation mimetic, his voluntary attention weak, etc. The frontal sinus guess holds its own without investigation. No one is in a position to say any-

thing worth while that is scientifically definite with regard to Negro character. And yet the need of such knowledge is imperative, no matter how the so-called Negro problem may be solved.

Locke's Place in the History of Thought. JNO. PICKETT TURNER.

Though there are many witnesses to Locke's wide-spread influence, just what Locke really taught is still an open question. A glance at his antecedents may help us to understand and more adequately set forth his doctrine. According to his own confession, it was Descartes who first awakened his interest in philosophical problems. But the influence of Hobbes, to whom he makes no reference, is probably more important. His association with Boyle and Sydenham in later years counts for much. Locke's name is unfortunately inseparably associated with the denial of innate ideas and a passive mind, which represent the true Locke very inadequately. The theory of innate ideas had already been undermined in a constructive manner by Hobbes, who undertook a scientific explanation of mind. The passivity of Locke's mind is, after all, a matter of misplaced emphasis. Locke's attack upon the doctrine of innate ideas is out of keeping with the spirit of the *Essay* taken as a whole. It was Locke's manner of expressing his objection to what he regarded false doctrines concerning the genesis of knowledge, which must come from experience. Because of the ambiguity of Locke's statements, we must have recourse to the *Essay* as a whole. Owing to the fact that Locke undertakes no explanation of mind, his sensationalism is untenable; in his discussion of the faculties of mind emphasis is continually laid upon non-sensuous factors. The mind is always the active organizer of experience; even in primary perception it is the organizer of brute sensations. This is in keeping with Locke's original purpose in writing the *Essay*. It is not denied that Locke's statements are frequently ambiguous. But it is maintained that it is more reasonable to interpret Locke as the precursor of Kant than of Condillac. The place that has been assigned Locke in our texts on the history of philosophy should be assigned to Hobbes. Knowledge cannot be thrust upon the mind ready made, but it must be gained by experience. This does not mean sensationalism for Locke, but simply empiricism. Locke was not free from intuitionalism, which he attacked, and no one more than he felt the need of what Kant regarded as the constant factor in our experience.

The Probable Influence of the Pythagorean Philosophy in Determining the Sequence of the Days of the Week. R. M. OGDEN.

It is well known that the days of the week derive their names from the seven planets of antiquity. In the supposed order of their remoteness from the earth, these planets were: the moon, Mercury, Venus, the sun, Mars, Jupiter and Saturn. But this is not the order in which the corresponding days of the week follow. When, however, we remember the Pythagorean conception of the 'heavenly heptachord' as applied to these planets, together with the fact that Pythagoras is reputed to have derived the relationships existing between the seven celestial spheres after the same formula by which he derived the intervals of the seven notes of the musical scale, we may reconstruct the order of the days of the week as we now have them.

The position of the sun in the celestial system with three planets on either side is directly analogous to the *Mese* of the Greek scale, which is the tonic with three notes above and three below. The derivation of these six notes consists in applying alternately the 'perfect' intervals of the fourth (3:4) and fifth (2:3), the fourth being always applied as a descending interval and the fifth as an ascending interval, *i. e.*, always starting from a tonic which is symbolized as 2, or a power of 2. If this procedure be applied to the planets, we shall start with the sun, or *Mese*, descend to the fourth below, the moon; ascend to the fifth above this, Mars; descend again by four to Mercury; ascend by five to Jupiter; descend by four to Venus and ascend, finally, by five to Saturn. This gives us the sequence which corresponds to that of the seven days of the week.

It may be noted that the so-called astrological derivation, of eastern origin, in which the hours of the day are each in regular order attributed to the seven planets, also accounts for the sequence we now use. Dio Cassius (b. 155 A.D.) mentions both accounts (XXXVII., 18, 19), but does not attribute the former to Pythagoras, nor does he clearly show the method of derivation to be the same as here described.

JOINT SESSION WITH THE CHILD STUDY DEPARTMENT, SOUTHERN EDUCATIONAL ASSOCIATION

Some First Measures Urgently Needed for Child Welfare upon the Part of Municipal and Educational Authorities of the South. DAVID SPENCE HILL.

After cataloging numerous issues of crucial importance today for

the welfare of children and women in the south, the author singled out two measures for the especial consideration of educators:

(1) *Medical Inspection of School Children and School Houses.*—The arguments for and against medical inspection were reviewed, and the movement for inspection heartily endorsed, attention being called to its educative and sentiment-creating value in addition to its direct benefits to the people.

(2) *State and Municipal Bureaus of Research.*—These bureaus are to take the first steps, *i. e.*, to get the data and to distribute the same concerning all matters pertaining to child-life in our cities and states such as infant mortality, diseases, degeneracy, illegitimacy, occupations, legislation, etc. These would not interfere with, but would rather aid the proposed Federal Bureau.

These two measures, if adopted, would mean the application of the scientific method of research to the roots of our troubles.

Child Labor in its Relation to Education. A. J. McKelway.

The figures of the census of 1900 are overwhelming in their proof that the child labor system is the fatal obstacle in the way of the education of children, and that in the factory villages where the employment of children is allowed, illiteracy among the children is most appalling. There are two reasons for this: first, the obvious reason that the mill is in competition with the school for the children, and second, that whenever the unskilled labor of children proves profitable, the incentive for an education, both to parents and children, is largely taken away. Census Bulletin 69 shows, for example, that the percentage of illiteracy for children from ten to fourteen years of age in cotton-mill villages is, for North Carolina, 50 per cent., as compared with the general white illiteracy of the state between these ages of 16.6 per cent. Similarly, in South Carolina it is 48.5 per cent. as compared with 14.8 per cent. for the state at large, and in Georgia it is 44.8 per cent. as compared with 10.4 per cent. That is, speaking generally, the illiteracy of the children of factory villages in North Carolina is three times as great as the illiteracy of the white children of the state at large; it is more than three times as great in South Carolina, and more than four times as great in Georgia. A recent investigation by the Bureau of Labor shows further that in the cotton-mill villages the percentage of illiteracy is increasing rather than decreasing. It is impossible to reconcile these results with the claim that the cotton mills now furnish an opportunity for the education of the mill children. The first duty, therefore, is the legislative prohibition

of such employment up to fourteen years of age, and the second, the enactment of compulsory school attendance laws for all the southern states.

Knowing and Expressing. R. M. OGDEN.

The argument of this paper is directed against the pedagogical doctrine that to know a thing implies the ability to express one's knowledge of it. The apparent psychological justification of this doctine is found in the combination of the 'structuralist's' or analytical psychologist's view that there are but three mental categories, namely, *sensation*, *image* and *feeling;* together with the view of the 'functionalist' that all states of consciousness issue in bodily adjustments, and are only complete after such adjustments have taken place.

The view of the 'structuralist' is objected to on the ground that recent experimentation has demonstrated the existence of concrete, imageless thought-contents. In case this assumption is substantiated, it no longer follows that thought must issue in relatively adequate expressions, since the imageless thought-contents are more or less indifferent to any particular type of expression. If thought consists primarily in images, there would seem to be no reason why these images should not be adequately expressed in the usual course of events. But if images are not essential to thought, the translation into linguistic or other forms of expression is a distinct process, and not directly consequent on thought.

The main body of the paper is taken up with a detailed examination of the introspective results obtained in an experiment on *meaning*, together with an attempt to demonstrate the inadequacy of the usual categories of sensation, image and feeling to deal effectually with the experience described.

In conclusion it is maintained that thought and expression are distinct processes, that adequate expression does not follow as a necessary consequence of clear thought, and that, therefore, more stress should be laid on the expressive side of education in an endeavor to cultivate the expressive art individually.

A Study in Rhythm Perception. KNIGHT DUNLAP.

Investigations so far have brought out a number of facts concerning rhythmic groups of sense-perceptions, but have not discovered the base of the grouping. The attention-wave theory, if restricted

to the rhythmised content, is obvious; if extended to the total content, it requires redefinition and proof. Certain of the motor theories may be descriptively accurate, but do not seem to get at the fundamental problem.

Extensive introspection has convinced the author that the rhythm-group is essentially connected with the specious present. All members of the group are 'present' at once to the observer's consciousness, and become simultaneously 'past' at the termination of the group. The specious present begins with the group, elongates as the group grows and contracts (or vanishes) at the end of the group, a new specious present taking its place. Introspection of other subjects is corroboratory, but introspection is not to be considered as sufficient evidence.

Experimental work in progress in the Johns Hopkins laboratory, which bears indirectly on the points raised by this theory, is covered by the following: (1) determination of the different thresholds for rate of stimulation; (2) determination of difference sensibility for internal time- and intensity-relations (several problems); (3) investigation of limits of effect of rate of stimulation on apparent intensity; (4) the state of consciousness in the intervals between groups: to be investigated if adequate means of attack are found.

Observations on Imitations in Raccoons. W. T. SHEPHERD.

The paper in some respects is supplementary to a paper 'Concerning the Intelligence of Raccoons' already published by Professor L. W. Cole, and to a paper on 'The Discrimination of Articulate Sounds by Raccoons' reported by the present writer. The same four animals were used as had been employed in the work above mentioned. At the time these observations were made the raccoons were seven months old. They had been trained on various tests but, with the exception of one individual, on none similar to those here reported. Comparative psychology defines three kinds of imitation: 'instinctive' imitation, 'gregarious' imitation and the higher order of 'inferential' imitation. The last type is exhibited when one animal sees another perform an act, its result, and then intelligently performs the same act. This type of imitation was studied in these tests.

An inclined plane of poultry wire, 4′–6″ long, served as apparatus. It was stretched across the corner of the room to a platform fastened to one wall, and had a step leading up to the end to be ascended. When a signal was given, the imitatee went up the plane to the plat-

form where he was fed. This was repeated several times, while the animal to be tested was able to see his reaction. Immediately afterward the other animal was tested for a sufficient time alone. The signal was given and all the conditions were as with the imitatee. The first raccoon was tested in this manner on two different days. He failed to imitate when left alone. However, in three of the trials with the imitatee he went up on the platform where the former was eat ng and smelled around. The second animal was tested similarly on two different days. She failed entirely to imitate when tested alone, but also, in one trial with the imitatee, went up on the platform and smelled around. The third raccoon was tested on two days. The first day he failed when tested alone after the imitatee had done the act 21 times. The second day in the first trial alone he went to the p atform in 2 minutes, 15 seconds. In the following nine tria s he reacted correctly, reducing the time to 11 seconds in the ninth trial. In the first trial that day he halted when part way up the p ane and looked at the experimenter. He cont nued to act in this manner up to the eighth trial. In the last two trials his reactions were prefect.

The results of the experiments appear negative. Both the first and second animals failed entirely when tested alone. The reactions of the third raccoon appear more doubtful. But his hesitating manner and the large number of trials required by him to perfect the reaction, indicate that he learned by mere 'trial and error,' and not by 'inferential' imitation. The writer therefore concludes that if 'inferential' imitation is a part of the raccoon's mental equipment, the experiments reported failed to disclose the fact. In some other tests for imitation made on the same animals and reported in Professor Cole's paper, only negative results were obtained. Observations on imitation in raccoons by Mr. Davis likewise yielded no positive results.

Class and Practice Experiments upon the Learning Process. DAVID SPENCE HILL.

This paper emphasized the need for a tried series of class-experiments, tests which will evoke the responsible activity of the elementary student in educational psychology, that will be intensive, problem-provoking and interesting. The suggestions of Seashore, Dearborn, McKeag and Hill (*Jour. Ed. Psych.*, 1910) for such tests were cited. The mirror-drawing test was used successfully with a group of five students to illustrate the 'trial and success' method of

learning in the case of adults. Slight modifications in Dearborn's apparatus were introduced, the arrangement being safer than that pictured in Whipple's Manual. The experimenter also introduced for tracing a combination of semicircles in addition to the star. The class tests were made individually in an adjoining room during the first quarter of an hour of recitation periods for two weeks. Data were also got from eight persons outside the class, one of whom made drawings during forty-seven consecutive days. Graphical representations were exhibited to show curves of improvement in time (seconds) and in errors. The typical curves agree with that of Starch in recording the most rapid improvement at the first trials. Modifying factors, physical and psychical, affected many minor variations. The time and error curves, obviously, were correlated. The tests were extended to demonstrate the transfer of practice effect; both similar (star) and dissimilar (semicircle) forms, as modifying the objective factors, being used. Unexpectedly large practice effects of initial and terminal tests resulted when a whole figure of either pattern was used for right and left hands. To complete in terminal tests the 'other half' of the same figures drawn in the initial tests is unsatisfactory because of the differences between the 'up' and 'down' movements. The same half of fresh figures would appear preferable, although certain incomplete groups of tests show marked differences in the first and second trials even in this last case.

As to the availability of mirror-drawing as a class experiment: (1) A more careful standardizing of materials and procedure is needed before attempting complication-experiments, such as the transfer of skill got by special practice. (2) To illustrate the learning process, and as introductory to a full discussion and study of it, these tests seem to be practicable. They are economical of time, simple in manipulation, bristling with interesting questions and meet many of the requirements for class-experimentation where small groups are engaged.

A Comparative Study of Children's Ideals. DAVID SPENCE HILL.

Data were got in answer to the printed question: "Which person (among those you have seen, or thought of, or heard of, or read about) would you most like to resemble? Why?" Precautions were observed to obtain spontaneous, individual responses. Complete analyses were shown by means of tables and curves for: (1) ideals at various ages (7–15) of 1,431 children; (2) ideals of Group I. (857 children), of Group II. (574 children); (3) ideals of 689 boys and of 742 girls

from the two groups; (4) analyses for the boys (430) and girls (427) of Group I., and for the boys (259) and girls (315) of Group II.; (5) the percentages in regard to Group II., calculated (*a*) by using actual number of papers, and also (*b*) by using 100 papers for each age. The writer did personally all the work by the first method, while a group of students analyzed the same data by the second method. There were no very considerable differences in the results.

The rubrics were: (1) *acquaintance*, father or mother, other relative, other acquaintance; (2) *public and historic persons*, president, historic Americans, historic foreigners, miscellaneous; (3) *characters from fiction;* (4) *religious characters*, God, Jesus, persons from the Bible; (5) *opposite sex chosen.* The actual content and the percentages were given; comparisons with similar studies made in Germany, England, New Jersey and California were considered. The results will appear in detail in the *Pedagogical Seminary.*

In conclusion, an appraisement was made of this type of 'human nature' study which, it was thought, should be classed under experimental pedagogy. Unavoidable difficulties are present; the psychological complex called an 'ideal' is not defined; the reaction of the pupil is peculiarly liable to imitation; the environment of the school doubtless influences the response; the minute comparison of curves is valueless; the age error is obvious.

Nevertheless, data in several similar studies steadfastly agree in attesting: (*a*) diminution of acquaintance ideals with age; (*b*) increase of public characters as ideals with age; (*c*) predominance of acquaintance ideals with girls; (*d*) small number of ideals from fiction and also (*e*) from religion; (*f*) girls choose more ideals from the opposite sex than do boys. The graphical representation of (*a*) and (*b*) is a useful illustration of the results of the developmental process as influenced by environment. The critique of statistical methods, the problems raised and the actual phenomena exhibited, all warrant the expenditure of moderate time upon this test by classes in education. Finally, in cataloging the actual persons from various sources mentioned as worthy of emulation, as well as the reasons given in answer to 'Why?,' the teacher-student is brought to face the problem of 'What' and 'Why' in education, as well as the mere 'How' of the experimentalist.

The Pressure Curve in Voluntary Control. JASPER C. BARNES.

This paper reports the results of one series of an experimental investigation of voluntary isolation of control in a group, a pre-

liminary account of which was read before the members of the society in 1909. In the series of tests reported in the former paper, the reagents frequently stated that they used pressure to inhibit the movement of the fingers. The apparatus described in the former paper did not register the pressure curve; so, at the suggestion of Professors Angell and Carr, we devised apparatus for that purpose. Plaster of Paris pistons, resting on coiled steel springs in glass tubes, contained pegs for the support of the little and middle fingers. The pistons were made to work air-tight by the use of oil. The glass tubes were connected with two tambours by means of rubber tubing. The slighest pressure on the pegs pushed the pistons down and thus moved the recorders of the tambours.

Two groups of subjects were selected. One group was instructed to relax and the other to contract the muscles controlling the fingers; both groups were instructed to move the ring finger through a distance of 2 cm. at the rate of two times per second. Fatigue usually began to appear at the end of one hundred seconds. Automatism was acquired more rapidly by the subjects instructed to relax than by those instructed to contract. The time required by the latter was twenty per cent. more than by the former.

The processes of inhibition and control were essentially the same as those given in the former paper. The pressure curve showed sudden, sharp, and irregular changes during the first stages of the learning process, but after inhibition was learned, the pressure curve remained practically constant and indicated a very small amount of pressure. The pressure curve of the subjects that were instructed to relax was more regular and less in extent than the curve of those instructed to contract.

During the learning process, a definite visual, tactual, auditory, muscular, tendinous, or articular image of the proposed movement initiated it. Frequently the image was of the mixed type. After automatism was gained, the signal alone was sufficient to touch off the innervated finger. When the hand is supported, pressure seems to be an important element of the process of inhibition during the learning process, but gradually decreases in intensity as automatism is approached.

The Modern Interpretation of Dreams and Visions. TOM A. WILLIAMS.

The type of thought in the dream is similar to that in the trance, vision, ecstasy, somnambulism and certain deliria. The differences from these states are circumstantial rather than essential. The

manner of association of ideas resembles that in false analogy where some fortuitous character is used as the basis of comparison. Folklore supplies plentiful examples. Mystical thought is often dominated by the false analogies derived from waking visions. It is not only mystical visions which influence people; those of ordinary dreams may do so. A remarkable instance is that of a patient of Flournoy who was saved from suicide by drowning by the vision of a friend who emerged from the water and reproved her. This type of self-communing, for such it is, does not differ psychologically from that used by automatic writers, and teleologically it fulfills the purpose of conversion, reflection, writing down one's ideas or any discussion which clarifies thought.

In some neuroses a strong factor is the morbid association of ideas such as occurs in dream thought, as, for instance, one woman could never enter a street car because she had once been almost suffocated in one. Accidental associations of this kind are a striking feature of dream thought. Symbols play a large part, as Freud has shown. His view that a dream is necessarily the gratification of an unfulfilled desire of childhood is not subscribed to; nor is his view that the external stimuli are of little importance in coloring dream thought. The experiments of the Marquis d'Hervey St. Denis show the contrary, as does also a recent example of Bernard Leroy. The former showed that every one dreams all the time, but that the substance of the dream is rapidly forgotton unless brought into consciousness by an associated idea. The pathology of dreams is illustrated by a case of Janet where a woman was prevented from sleeping by horrible hypnagogic visions of the death of her father and brother from typhoid fever. Actually, she had suffered great grief from the death of her child from this cause some months before.

Lastly, the relation of dream thought to the nature of perception and delusion is briefly discussed, the lowered attention of sleep being shown to play a large part in the phantasmagoria of dream life. The noetic quality of the dream is illusory, and its logic is false until interpreted. The interpretation requires great psychopathological knowledge and analytic refinement. The value of the procedure to medical art is witnessed in the effect of the treatment of psychogenetic affections on the pathogen, to which the analysis of dreams is a useful passport.

Intellectual Precocity: Comparison between John Stuart Mill and the son of Boris Sidis: Principles and Methods: the Rôle of Affectivity and Inclination. TOM A. WILLIAMS.

From various facts and considerations adduced, the author's conclusions are as follows:

I. Proper intellectual education inevitably leads to precocity as compared with the present average level of attainment.

II. In achieving this the prime factor is the maintenance of an affective disposition of satisfaction in performing the work needed for intellectual training. The conditioning of this depends upon the following principles:

1. The preservation of a healthy physical basis for agreeable feeling tone.

2. Seclusion of interest towards the task imposed by preventing its undue dissipation upon extraneous frivolities.

3. The stimulation of interest during the performance of tasks of pure drudgery through the affective pleasure given by (*a*) the feeling of activity for its own sake, (*b*) the feeling of accomplishment, (*c*) the desire for praise—to be used but sparingly—, (*d*) the desire to excell—a motive to be strictly subordinated, and used only occasionally—and (*e*) the fear of punishment. The use of the last is a pedagogic failure, but it is sometimes necessary on account of our own imperfections.

4. The graduation of study in accordance with real psychological necessity, and not in response to a hierarchy largely derived from the imagination of philosophers. This provision, however, is of less importance than many believe, provided the principles discussed are borne in mind.

5. The avoidance of stress and anxiety in the performance of work. Such an attitude indicates unwise interference. A child's natural intensity does not require stimulation, and his mental processes should not be hurried, but guided.

III. As intellectual pabulum must be provided for the growing activities of the child's mind, this should be selected and furnished in such a way that each acquisition may be a stepping-stone to something else in a clearly conceived plan of accomplishment.

IV. The director should never forget that the acquisition of new powers is slow. Practice should be frequent, but not unduly sustained. Accuracy is essential. Results should never be measured by quantity of apparent work. The criterion is intensity of application in a fruitful direction.

Learning and Forgetting: President's Address. EDWARD FRANKLIN BUCHNER.

The problem of learning has long awakened interest. Some Greek philosophers named some of the emotional factors in its solution. Formal logic regarded learning only as a procedure in terms of concepts, it being the office of memory merely to store away these results. Analytical psychology came forward, saying all the intellectual faculties are involved, although the fundamental 'law' is that the learner is to attend to that which is to be learned. The classic experiments on remembering and forgetting by Ebbinghaus reconstructed the analysis of memory, advanced experimentation beyond the limits of sensations and movements, and the problem of learning made its specific modern appearance. Further adjustments resulted among the many advantages of the sensory-motor conception of experience. Learning is linked up with complex processes, and finds an external register. Finally the genetic and comparative points of view enable one to view it as one of the typical organic expressions of mind. It seems to be a species of 'behavior' which mechanizes itself by the elimination of the useless as the organic scale is ascended. It is also a constant mode of psychophysical adjustment in which consciousness plays an increasingly important rôle. The character of education is determined by the central position learning occupies in experience.

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NOTES AND NEWS

Sir Francis Galton died January 17, in his eighty-ninth year. Of his various and varied writings perhaps the best known to the psychological world are his *Hereditary Genius*, published in 1869, and his *Inquiries into Human Faculty*, published in 1883.

News has been received of the death of Wilibald A. Nagel, professor of physiology in the University of Rostock, at the age of forty years. Working mostly in the domain of sense physiology, Professor Nagel was much interested in the psychological side of his studies and was always exceedingly hospitable to those American students of psychology who had the privilege of working under him.

The New York Branch of the American Psychological Association held its midwinter meeting at Columbia University on February 3 and 4. An attractive program, covering three half-day sessions, had been arranged by the secretary, Professor R. S. Woodworth. During the evening of the first day a special session was held in memory of William James. Affectionate and impressive recitals of personal reminiscences, mingled with critical appreciations of James's work and personality, were listened to by a quite unusual gathering of upwards of fifty professional psychologists and philosophers.

A course of nine public lectures on 'Problems of Psychology' was given at Columbia University between January 31 and February 9. Many of those who attended the meeting noted just above were fortunate enough to hear at least two of the following lectures: 'Memory and Imagination,' by Professor E. B. Titchener; 'The Frailties of Imageless Thought,' by Professor J. R. Angell; 'Psychology as Basis for the Social Sciences,' by Professor M. W. Calkins.

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THE PSYCHOLOGICAL BULLETIN

GENERAL REVIEWS AND SUMMARIES

SENSATION (GENERAL)

BY PROFESSOR MADISON BENTLEY
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The problem of the elementary processes, their nature and number, seems likely to remain open so long as psychology is constructive. For a decade, the question of the alleged 'thought-element' has held the attention of the laboratories. At the same time, the 'formal' elements of perception and ideation, which accumulated a large literature in the nineties, have not been entirely neglected. A. Gelb, of the Berlin Institute, has recently written a general critique of them (2). He appends a useful bibliography of forty-three titles. Gelb reviews the *Gestaltqualität* from the time of Ehrenfels to Marty and Kreibig. He takes up a dissenting attitude toward Ehrenfels, and he rejects the *fundierte Inhalt* of the Austrians. He contends, however, for the specific character of the *relation* which he sets down as 'given' with the ordinary 'absolute' elements of sensation. It may be questioned whether the substitution of 'relational' for 'formal' elements marks an advance in empirical psychology.

Semon (3) formulates a definition of stimulus that is consonant with his doctrine of the *Mneme* and the *Engram*. Stimulus is an operation upon living organisms which 'excites' organic substance and which produces a 'reaction.' Reactions are *sensory* (the stimulus is followed by sensation) or *objective* (an organic change, such as movement, growth, or elimination, ensues). Stimulus and excitation are factored in terms of energetics and a one-to-one correspondence is found to obtain between the factors. Stimulation does not necessarily involve kinetic energy or even the presence of an energetic agent (cf. the sensation gray and the absence of light). It is enough

that the stimulus should produce an organic change that 'releases' and maintains the excitation.

Thompson found (4) that when the head was placed within the field of a powerful magnet with alternating current a weak flickering light appeared in the retinal field. The sensation was observed to be more brilliant in the dark than in the light and to effect most strongly the peripheral portions of the field. Upon repeating the experiments, Dunlap noticed (1) that the flicker was strongest when the side of the head was presented to the coil and he suggests that the effect may be due to induction currents set up in the optic pathway. He is not certain that "there is a definite *arousal* of visual sensation by the alternating field; the effect appears more like an alternate intensification and inhibition of whatever sensory process is already in progress."

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VISION—GENERAL PHENOMENA

BY PROFESSOR EDWIN B. HOLT

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After a preliminary survey of earlier determinations in wave-length of Hering's four Urfarben, these having been made by several different methods, Westphal (10) proposes the questions: how far the Urfarben can be determined by direct judgment of *pure* color-quality, and then, how far persons will agree with one another on this point. This direct method has been previously used by Donders, L. von Kries, and E. Schottelius. Westphal finds his observers able to make the specific judgment on Urfarbe 'mit ziemlicher Sicherheit und grosser Konsequenz,' although this is subject to certain cautions (p. 227, § 5). Observers differ most markedly as to the place in the spectrum of primary red and green, they differ less as to yellow, and least as to blue, locating this fairly definitely at 478.8–479.4 $\mu\mu$. One observer's yellow lay only 14–24 $\mu\mu$ away from the green of another; and the distance between the Urfarben, in $\mu\mu$, is

not the same for different observers. As the intensity of a spectrum is increased the distance between primary red and green increases, the former moving to the left, the latter to the right; but this is due to the observer's tendency to select green and red both with a slight tinge of yellow, which yellow shows out when the intensity of the spectrum is increased (p. 228). Primary blue and yellow shift very little if at all as the intensity is changed. The admixture of white light to the colored affects the position of the Urfarben just as does reduction of the physical intensity. The mixture of a person's own primary red and green looks to him yellowish instead of gray; and so too his blue and yellow look greenish. The point of the spectrum which appears neutral to the deuteranopic eye appears bluish to the normal eye. Sometimes an observer reports, in locating say primary red, that he has to choose this not where the red looks *neither* bluish nor yellowish (for he finds no such point), but where it looks *both* blue and yellow in equal degree: similarly primary yellow sometimes looks both red and green. This last point bears interestingly on some contentions of Brentano.

Westphal's observers find that the extreme red end of the spectrum begins to have a bluish cast, as indeed Maxwell has described it to have. Rayleigh (**9**) now reports, in agreement with König and Dieterici, that this is not the case; the farthest visible portion of the red end remains purely red. From this, then, Rayleigh would probably locate his Urrot at the farthest visible wave-length, in opposition to Westphal. Rayleigh further reports that while most persons of normal vision (himself included) see a reddish cast in the violet end of the spectrum, yet not a few see nothing here which at all resembles red; and this abnormality, he finds, is not connected with color-blindness. He drops this interesting hint (p. 205): "That ordinary normal vision is very approximately trichromic cannot be doubted; but a question may be raised as to the possible existence of a very subordinate fourth element of color."

Dittler, Orbeli, and Richter take up three further points connected with earlier investigations of Hering. Dittler and Richter (**2**) confirm, by a new method, the statement of Hering and Brückner that if a part of the retina (either foveal or extrafoveal) is fatigued for white light, a colored stimulus is seen on this spot less intense but more saturated than on an unfatigued part of the retina. If the intensity of the colored stimulus is increased on the exhausted portion until it here looks as *bright* as the original intensity on the unexhausted portion, it will also still look very much more *saturated.*

The two sensations can be made much more nearly (and indeed practically) identical if on the white-fatigued portion the intensity of the colored stimulus be left unchanged and white light be added to the colored until the sensation from this field is as bright as the sensation from the colored stimulus alone on the other, unfatigued part of the retina. The experiment is thought to show, as against the Helmholtz theory, that the white visual process is fatiguable independently of any color process, and that the former is therefore independent of any color process.

Dittler and Orbeli (3) test once more Fechner's assumption that excitation is proportional to stimulation. If this is true of vision it should follow that if the stimuli for two unequally fatigued spots of the retina have had their intensities so adjusted that they yield equally bright sensations, these sensations should remain equal in spite of changes in the absolute intensity of the two stimuli so long as their relative intensities remain unchanged. This hypothesis has been reported verified, and is now called by J. von Kries the 'Koeffizientensatz' (cf. *Nagel's Handbuch*, 3, 211) and by Wirth the Fechner-Helmholtz theorem. In 1873 Hering reported an experiment which seems to disprove this theorem, and the present authors confirm Hering. They find that brightness equations, established as above described, vary appreciably if the absolute intensity of the two stimuli is increased or decreased, by say 25 or 30 per cent., although the relative intensities are undisturbed.

When the eye is stimulated briefly by a moving light, a positive like-colored after-image is seen between the primary image and the so-called Purkinje after-image. If a system of two moving lights is used (as say two openings in an opaque movable screen), the conditions can be so regulated that this positive after-image of the foremost light coincides with the primary image of the second light. Three images are then seen following one another in space: (1) a primary image of the first light; (2) an image formed from the superposition of the positive after-image of the first light and the primary image of the second light; (3) a positive after-image of the second light. This is Hering's 'three-image phenomenon,' and it has been studied afresh by Orbeli and Dittler (8). These experimenters find that the color and brightness of the second (compound) image depend on the color and intensity of both stimuli, while the third image depends on the color and brightness of the second stimulus *alone*. Thus if the foremost light is intense red and the second light a less intense green, a green after-image (the third image as described

above) will be had as the 'positive and like-colored' after-image of a stimulus whose primary image (the second as described above) has been altered to a dull red.

Goldschmidt (5) undertakes to measure the brightness of positive after-images by comparing them with the simultaneous primary image from a second stimulation. It seems incredible that this second stimulation should not appreciably modify the brightness of the after-image to be measured, and yet Goldschmidt states that it matters not at all whether the comparison stimulus is presented just before, simultaneously with, or just after the rise of the positive after-image. Other remarkable conclusions are: that visual and general fatigue do not effect the brightness of positive after-images although they do affect the general accuracy of an observer's judgment; that considerable differences in the state of adaptation of the eye have very little influence on the brightness of positive after-images (for a decided dark-adaptation the after-image is somewhat brighter); that the brightness of an after-image is almost independent of the color of the stimulus; and that if the brightnesses of different positive after-images are compared with simultaneously presented comparison stimuli, the relative difference threshold is found to be constant. It lies between one-fifth and one-tenth, *and* is three or four times greater than the relative difference threshold for two simultaneous primary images. Further, and more readily credible, conclusions are these: that the intensity of the stimulus increases more rapidly than that of the positive after-image; that the brightest moment of a positive after-image follows its stimulus more closely the intenser the stimulus is; that on a given day the judgments of almost every observer vary in the same direction from (above or below) that observer's own total average. The author ascribes this last to the effect of light and dark weather on the idio-retinal light, which in its turn importantly modifies the positive after-image.

Flügel and W. McDougall (4) publish further determinations of that which McDougall has previously called the 'action-time' of light, *i. e.*, the time of exposure which gives the maximum stimulation effect of a given light intensity. The action-times found, varying between 50 σ and 200 σ, are not significant because the intensities that were used were not measured. "When the action of light upon the retina is prolonged beyond its action-time the intensity of the sensation declines very rapidly, and more rapidly the more intense the sensation, the more intense sensation declining to 50 per cent. of its initial value in about one-fortieth of a second" (p. 206). Inas-

much as the intensity at any moment of a so rapidly changing sensation could not be judged with any accuracy, and from other statements of the authors as well, the reviewer supposes this to mean that the maximum intensity produced by an exposure that exceeds the action-time is less than the maximum produced by one of action-time length: if so, it follows that either the as yet ungiven continuation of a stimulus already exerts an inhibitory effect on the stimulation that has been given, or else the rapid decline in the sensation prevents the observer from *passing judgment* on what has been the maximum of the sensation. This latter is surely the more probable, and thus it should seem that the value of this investigation lies not in its bearing on action-time as related to brightness, but in its bearing on the process of (brightness) judgment. The authors state that the difference threshold is higher when the comparison is simultaneous than when successive; this they attribute to "falsification by physiological 'contrast.' " They mean 'simultaneous contrast,' and do not deem successive contrast to be so mischievous. It is further stated that in rod-vision the reduction of duration of a stimulus is equivalent, so far as the intensity of sensation goes, to reduction of the intensity of the stimulus, that is, the Talbot-Plateau law holds for rod-vision.

Although the field of a telescope or spy-glass never presents its objects so intense as the same objects are when viewed by the naked eye, yet the telescope field ordinarily, specially at twilight, looks brighter. Why? A. Müller (7) attributes this partly to the contrasting black border which is seen around the telescope field, but more specially to some considerations connected with the objective brightness differences given in the telescope field, and with Weber's law. These considerations are not given in further detail because they seem to the reviewer to be fallacious.

Babák (1), from work on amphibian eyes, finds evidence of 'qualitatively' opposed (antagonistic) retinal processes. Both light and darkness act as positive stimuli: and the retina when not exposed to light still influences, through the central nervous system, the tonus of the body, et cætera. In this connection Liesegang (6) describes a chemical combination which alters much more rapidly in darkness than in light, *i. e.*, which is 'affected by' darkness rather than by light. The process of change is described which this substance undergoes when exposed to darkness. Liesegang believes that this discovery bears interestingly on the question whether black is a sensation.

Winch (11) discusses at length the relation between the development of color terminology in children and the development of their sensibility to different colors. He finds the 'order of the correct

application of color-names' in children from three to five years old, to be: black-white, red, blue, green, yellow, violet, orange. He suggests that this is perhaps the order in which color sensation, at first unitary, gradually differentiates. Winch also presents (12) a study of the color preferences of London school children and others, as found by asking them to write down the *names* in their order of preference. The results are:

Youngest girls	(7 to 10 yrs.)	red	blue	yellow	green	white	black
" boys	" " " "	blue	red	"	"	"	"
Older girls	(10 to 15 yrs.)	"	"	green	yellow	"	"
" boys	" " " "	"	"	"	"	"	"
(21) Adolescent males	(15 to 20 yrs.)	"	"	"	white	yellow	"
(24) Men	(20 to 39 yrs.)	green	blue	red	"	"	"
Women teachers	(18 to 46 yrs.)	blue	green	white	red	"	"

There is some slight indication that color preferences are more closely correlated with general mental development than with age irrespective of that development.

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VISION—COLOR DEFECTS

BY PROFESSOR SAMUEL P. HAYES
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The question of color tests is attracting considerable attention in England. Judged both from the general results of the tests and from the difficulty of deciding in specific cases whether a man is unfitted for service in the merchant marine because of color deficiency, the tests at present used by the Board of Trade must be very faulty or very badly conducted. *Nature* (2, 7) gives an interesting account of the case of Mr. John Trattles, who in six color tests during the last six years failed three times and passed three times, and was finally declared competent to act as second mate, by a special civil court, upon the testimony of fellow seamen who had made practical tests of his ability to 'pick up the lights,' against the unanimous verdict of a group of scientists that Mr. Trattles was certainly color-blind. The general efficiency of the tests may be judged by the statement made in the House of Lords during a debate (2) upon the need of an investigation of the tests used, that in the four years 1905–09 out of 25,151 candidates examined, 212 men were rejected, some of them upon a second test. As there seems to be overwhelming evidence that at least 4 per cent. of all white men are color blind, one would expect to hear of over a thousand rejections; and if the anomalous trichromates as well as the color-blind were excluded from such service, as in Germany, the number of rejections should be very largely increased. A committee to look into the whole matter of color tests is announced in *Nature* for June, 1910 (p. 529).

During the year a number of persons interested in the subject have published suggestions for improving the methods of color testing (3, 5, 9). The Holmgren test which seems (5) to have been the chief test used by the Board of Trade since its recommendation by the committee of 1890 (6) is attacked because it catches men who are not color-blind and fails to catch those who are (6), because it is illogical (5) and unjust, etc. Various lantern and out-of-door tests are recommended (6, 9, 15). Naming tests are judged to be fairer and surer than matching tests (9). Allen (1) recommends the method of persistency curves as a final test in disputed cases.

H. M. (11) suggests that color-blind seamen might be enabled to pick up the lights successfully if they were provided with colored glasses through which they could view signal lights, since through a red glass a red light could be seen while a green one would disappear, etc.

In his new *Manual of Mental and Physical Tests* (14), Whipple devotes a section to color-blindness, giving careful directions for the use of the Holmgren worsteds and the Nagel Tafeln.

Stilling (13) gives an illuminating discussion of the known anomalies of color vision from the point of view of pathological ophthalmology, and briefly outlines his general position in the introduction to the new edition of his *Pseudo-Isochromatische Tafeln* (12). From an extended comparison of many cases of congenital and acquired color-blindness, he comes to the conclusion that all color defects are pathological, the so-called congenital cases being the result of affections in the optic tract during early years, or during the fœtal period. Congenital total color-blindness, which is practically identical with acquired total color-blindness, seems clearly pathological on account of the visual defects which accompany it (nystagmus for example); congenital partial color-blindness is closely analogous to acquired partial color-blindness, except that in the latter, visual acuity is more likely to be affected. Stilling finds it easier to prove this analogy in the case of red-green blindness, since that is so much better known; but he feels confident that congenital blue-yellow blindness (of which he has studied *nine* cases) will show a similar analogy to acquired blue-yellow blindness. The color anomalies which it has been customary of late to designate as anomalous trichromacy are either cases of color-blindness in which the subject has learned to distinguish many colors by secondary criteria, or cases of sub-normal sensitivity to red and green, a state of color amblyopia similar to that which often occurs before temporary red-green blindness. He lays special emphasis on the fact that most of the work upon anomalous trichromates has been conducted and described by persons who are themselves more or less defective in color vision.

Stilling highly recommends tests of color contrast for the determination of color-blindness, and the production of permanent color-blind equations by mixing pigments.

His comparative study of congenital and acquired cases leads Stilling to the following conclusions in the psychology and physiology of color:

1. There must be in the visual apparatus at least three systems of specific visual energy,—a brightness system, a red-green system and a blue-yellow system.

2. In normal color vision there are four fundamental colors,—two pairs of antagonistic colors.

3. In partial color blindness, the specific energies necessary for

one of these pairs are lacking, leaving a color system reduced to one pair of colors.

Köllner's study of acquired red-green blindness (10) leads him to conclusions which differ somewhat from those of Stilling. Köllner finds a fairly distinct and definite type of dichromacy in certain disorders of the optic nerve, but he does not think the defect so closely similar to congenital red-green blindness as Stilling claims. His experiments upon one case in which he watched the progress of the disorder from normal color vision to complete dichromacy, and his work upon many other cases in different stages of color deficiency, lead him to posit the following characteristics as typical of acquired red-green blindness: (1) a two color system whose warm curve is intermediate in form between that of protanopia and deuteranopia, while the cold curve resembles that of normal color vision; (2) a series of color sensations like those obtained from the blue-yellow zone of the normal eye; and (3) a brightness distribution like that in normal daylight vision.

During the development of the defect there is, perhaps, an early stage similar to that found in anomalous trichromacy; the first stage of which he is sure, however, is a condition in which yellow and green are confused, but yellow and red distinguished; in the second and final stage, all the colors from red to the neutral band in the blue-green are confused, and distinguished only by differences of brightness. Acquired red-green blindness is, then, a 'reduction form' from normal color vision, like the familiar congenital forms of dichromacy.

Guttmann (8) reports that, in a series of experiments with the Helmholtz spectral color-mixing apparatus, four anomalous trichromates of the green-weak type regularly obtained blue negative after-images from all lights in the warm end of the spectrum. As they seemed to get gray after-images from pure green and its complementary purple given simultaneously, and yellowish after-images from blue and violet, he concludes that he has added another proof of the close relationship between anomalous trichromates and dichromates, though he confesses himself quite unable to give any theoretical explanation of the fact that in its reaction to spectral lights the anomalous retina appears to be trichromatic, while its after-images are those of the typical dichromate.

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VISION—PERIPHERAL, FOVEAL, ETC.

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The literature on this topic for the year is scattered.

Stratton (4) discusses the localization of diasclerotic light, taking his start from Veraguth.[1] Veraguth claims that sensations aroused by light entering the eye through the sclerotic coat are often localized upon the side on which the stimulus falls. This was found to be generally true of the temporal side of the eye with the exception of a few cases in which, in addition to the sensation referred to the temporal side, there was a weaker sensation which was referred to the nasal side. It was not true at all of the nasal side of the eye. Here the localizations were made in the usual way to the opposite side of the field of vision. Veraguth established to his own satisfaction that the stimulation of the retina was in each case on the side on which the stimulus was given, for the effect was gotten when the incident light was tangent to the surface of the sclerotic. In this case, he reasons, it could not pass through the eye and stimulate the retina on the opposite side. He believes that we have here an indubitable departure from the usual cross-localization of visual impressions, and an argument against the nativistic theory of visual space. For, he

[1] Veraguth: Die Verlegung diaskleral in das menschliche Auge einfallender Lichtreize in den Raum. *Zsch. f. Psychol. u. Physiol.*, 1906, **42**, 162–178.

reasons, if the space value of the impression inheres in the retinal element we should not get such different results from stimulating the same elements by light coming in the one case through the pupil and in the other through the sclerotic coat. He explains the localizations on the temporal side by the principle of biological utility. They are just what should be expected from this principle. The need of the organism is that the impressions be referred in the direction from which the stimulation actually comes. This would not be fulfilled were the localization made on the opposite side. The principle of utility has not worked itself out on the nasal side because so large a part of the field of vision on that side is shut off by the nose. Diasclerotic and diapupillary stimulation thus have each developed a different group of factors to determine their localizations. With some persons it is not improbable that both sets of factors are aroused by diasclerotic stimulation. In this way he explains the double localization which he noticed in some of his experiments.

Stratton finds that there are regions on the temporal side where the localizations are (1) to the temporal side of the field of vision, (2) to the nasal side, and (3) to both sides. The first begins immediately posterior to the edge of the iris and extends back, roughly speaking, to the ora serrata. There are no rods and cones in this region and the light passes through and stimulates the retina of the opposite side. The localization to the temporal side is as it should be, thus, by the principle of cross-localization. The second region lies still further back. The light here stimulates the rods and cones lying beneath its point of incidence, hence the localization is on the nasal side. The third region is found at the junction of the first two. Here we might expect the retina on both sides to be stimulated, hence a reference of the images to both sides of the field of vision. On the nasal side, he finds that all the localizations are on the temporal side of the field of vision. He accounts for the absence of regions in which the localization is on the nasal side in two ways. (1) The ora serrata extends farther forward on the nasal than on the temporal side. (2) Light entering anterior to the ora serrata, instead of passing through the eye and stimulating the retina of the opposite side, may strike the surface of the lens near its periphery and be reflected back to the nasal side posterior to the ora serrata.

Stratton sees in his results nothing necessarily contradictory to the principle of cross-localization.

Hubbard (1) describes "a curious secondary visual phenomenon resulting from a stimulation of the macular region." This phenome-

non may be observed with any feeble source of light viewed against a dark ground; but it is best seen when a patch of soft red color, such as is given by a ruby lamp wrapped with tissue-paper, is observed in a dark room. When so viewed two diffuse streamer-like brushes of bluish tint are seen to spread laterally from the lamp,—to the right in the right eye and to the left in the left eye. The brushes enclose a neutral space irregularly oval in shape; and terminate at one end in the macula and at the other in the blind spot. At the blind spot they are narrow and line-like. They have their greatest breadth midway of their length. At the macula they are of medium breadth. The following points may be noted: (1) The clearest effects are given by a narrow stimulus aperture. (2) The brushes are always blue in color whether the stimulus be monochromatic or white light. (3) Their intensity increases with an increase in the intensity of the stimulus light and varies with its wave length. The effect is weakest at the extreme ends of the spectrum, is strong in the yellows, and reaches its maximum at orange-red. (4) The phenomenon strongly shows the effect of fatigue. With the eye fixed in one spot, the effect dies away in two or three seconds. The brushes are best seen when the light is turned on suddenly, in which case they flash out at once, beginning at the macula and extending quickly to the blind spot.

The writer concludes that there is some connection between the phenomenon and the distribution of the fibers of the optic nerve between the macula and the papilla. It would seem quite possible that certain excitations passing from the macular region along these fibers to the papilla are capable of inducing secondary effects in the portions of the retina through which they pass.

Rayleigh (2) reports an investigation conducted by him of the sensitivity of the eye to variations of wave length in the yellow region of the spectrum. He was prompted to this work by Edridge-Green's[1] method of determining the number of separate parts or divisions in the spectrum within each of which the observer can sense no color difference. Edridge-Green's method is as follows: Movable screens are provided in the focal plane of the spectroscopic telescope by means of which the breadth of the spectral section admitted to the eye is limited. Beginning at the extreme visible red, more and more of the spectrum is admitted, until a j.n.d. of color is obtained. This gives the first division. Starting again from the terminus of this division the whole spectrum is gone over. Working by this method the normal eye distinguishes about 18 of these divisions which Edridge-Green

[1] *Roy. Soc. Proc.*, B, 1910, 82, 458.

calls monochromatic. Edridge-Green uses this apparatus to test color sensitivity or its converse, color blindness. When tested by Edridge-Green, Rayleigh was able to distinguish only 17 changes of color tone in the spectrum. In the yellow for example a transition of 595–586 was required.

Rayleigh considers this method of testing color vision very insensitive. Working with the color box (after Maxwell) described by him in 1881,[1] he found that in the yellow region of the spectrum, he was able to distinguish as different the interval between the *D* lines which formed about one fifteenth of the division he called monochromatic by the Edridge-Green test. He points out two probable defects of the Edridge-Green test. (1) The gradual character of the transition is an obstacle to the recognition of the difference. There is a tendency for the next preceding color on which the eye has rested in going from one end of the division to the other, rather than for the standard color, to form one of the terms of each comparison. (2) Too small an area of the retinais stimulated by light of the same wavelength. The colored fields compared should have considerable area.

Scrini (3) discusses the 'mouches volantes.' His article covers the following points: (1) The literature is reviewed to the present time, beginning with Maître Jan, 1707. (2) Four sets of observations are given, selected from his own clinic as typical. Specks, lines, irregular blotches resembling dendrites, and patches made up of complications of a part or of all of these are described as seen floating before the eyes. (3) The phenomenon is discussed in relation to entoptic appearances (globules, fibers, etc.) ascribed to opacities in the humors of the eye.[2] By casual observation the connection is not obvious, but by a close analysis, the lines, specks, etc., characteristic of the phenomenon described by Scrini's subjects may be detached from the masses forming the entoptic appearances. (4) The phenomenon is also discussed in relation to neurasthenia and various nervous disorders accompanied by morbid introspective tendencies. Scrini points out from his own observation and from the literature that these appearances can be observed by practically every one when the observation is attempted under the proper conditions and after the subject's attention has once been drawn to them. The phenomenon, then, is normal. Two reasons are given for the frequency with which it is

[1] *Nature*, 1881, **25**, 64–66; Scientific Papers, **1**, 453. Vide also *Nature*, Aug. 18, 1910.

[2] Donne (*Arch. gen. de Med.*, 1830, **23**, 113), for example, reports that opacities corresponding in shape to these appearances are found under the microscope in the aqueous humor of the dead eye.

reported by the neursathenic: (1) He has a hyper-sensitive retina, hence senses the phenomenon more strongly than the normal person. (2) His attention is morbidly predetermined towards subjective phenomena.

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HEARING

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The year 1910 has brought us at least two publications which set forth experimental results of prime importance for the psychology of hearing. It is interesting to note that both emanate from the Berlin laboratory, indicating that we must still look to this quarter for most of the major work being done in this field. One of these publications is by the director himself, a very painstaking study of the phenomena of combination tones (8), while the other, by Köhler, bids fair to bring about a small revolution in our conceptions of tonal quality (3).

Stumpf's investigations were inspired mainly by his various controversies with Krüger concerning the significance of difference tones for the phenomena of consonance. In considering his results it will be well to review briefly Krüger's contentions, the critical presentation of which, as it happens, has reached its rather long drawn-out conclusion within the year (4)—and almost simultaneously with Stumpf's renewed attack.

Krüger, it will be remembered, bases consonance in the main on the coincidence of two or more difference tones, provided that the total number of difference tones present must be relatively small. He thus prepares the way for a series of combinations which show all grades of consonance and dissonance, and also at times a condition of neutrality. For instance, he argues that the position of an interval in the scale, with reference to absolute pitch, introduces alterations in the 'sonance' character to such an extent that only a few

intervals retain their consonance intact when the pitch is varied. For most intervals a more or less limited region of the scale alone satisfies their consonance requirements, above that they tend towards neutrality, and below it towards dissonance. This fact he bases on physiological and other conditions which render the difference tones more effective in one region of the scale than in another.

Krüger's complicated analysis refers not merely to the difference tones of 'first' and 'second order,' which are commonly accepted. His investigations have led him to conclude that there are clearly perceptible at least five orders of difference tones. These may be derived by a process of continued subtraction which starts with the numbers expressing the relation of the interval and continues by taking each time the difference of the two smallest numbers among those of the objectives and difference tones present.

It is to this question of the five orders of difference tones that Stumpf now turns his attention. After an investigation which is a model for exact and delicate manipulation of instruments, and steady unbiased observations, he arrives at the following results: The formulæ for all possible perceptual combination tones may be thus stated: (1) $h-l$, $h+l$. (2) $2l-h$, $2h-l$. (3) $3l-2h$, $3h-2l$. (4) (probable) $4l-3h$, $4h-3l$.

In the above h signifies the higher objective tone, and l the lower objective.

These formulæ are limited, however, by the following facts: The only intensive difference tones are those expressed by $h-l$, when the objectives are within an octave, and $2l-h$, which only exists under this condition. Intervals within the minor third alone produce the difference tones indicated by orders 3 and 4 of the formulæ. Beyond the minor third there exist only the combination tones of orders 1 and 2. Above the major sixth the $2h-l$ is lacking; above the octave there remain only $h-l$ and the summation tone $h+l$; beyond the ratio 1 : 8 there is only the summation tone and beyond 1 : 12 there are no combination tones.

Judging from the nature of Stumpf's methods and execution, this would seem to dispose rather effectually of Krüger's elaborate hypotheses, and with them his attempts to base consonance exclusively on a perceptual basis. This is the more evident inasmuch as Stumpf's investigations indicate that combination tones are always directly derived from the objective tones, and not from beats nor, except in the highest ranges of the scale, from one another as Krüger maintains. Neither does Stumpf find any evidence for the construc-

tion of 'mean' tones among the difference tones; a phenomenon which, according to Krüger, helps to make an interval rough-sounding whenever two or more difference tones chance to be near one another in pitch.

As a striking culmination to the critical presentation of his own theory, Krüger cites Riemann's fatal objection to Stumpf's fusion theory. The objection is based on the apparent impossibility of explaining by this means the dissonance of the triad c–e–g♯, wherein all the binary intervals are consonant, namely, two major thirds and a minor sixth. According to Krüger's theory the dissonance of this chord is clearly indicated by the character of the difference tones which are produced. Considering the tempered intonation, these are, indeed, all different and may well contribute to the unsatisfactory result. Krüger fails, however, to note that this same tempered intonation is responsible for considerable impurities in the several binary combinations. If we assume, as he does, $c' = 256$ vib., $e' = 323$ vib. and $g\sharp = 406$ vib., we find the last note standing for either 403.75 vib. in its relation of 4 : 5 with 323 vib., or as 400 vib. in the same relation to e′ as the latter should be in just intonation (320 vib.); while, on the other hand, it must stand for 409.6 vib. in order to fulfill its relation of 5 : 8 with c′. It is one thing to maintain that intervals which fuse naturally must continue to do so when they enter combinations with another related tone, but surely it is quite another thing to require that this shall hold true when the tone in question is a compromise between two tones each of which bears a distinct relation to each of the tones of the original interval. It would, of course, be impossible to produce this triad with the relations noted in just intonation, and it seems quite possible, to the writer at least, that the chord in tempered intonation is peculiarly suited to emphasize by contrast the unsatisfactory result of requiring one tone to do duty for two.

We are thus apparently thrown back upon some physiological disposition which may be regarded as conditioning the phenomenon of consonance, a supposition which has always been fundamental to Stumpf's theory and one which the writer, among others, has attempted to make plausible.[1] Not that difference tones and overtones may not be regarded as striking accessory features, as they doubtless are within their respective limits, but that any attempt to base the phenomenon of consonance on purely perceptual evidence would appear to be demanding overmuch of the perceptual complex.

[1] Psychological Bulletin, 1909, 6, 297–303.

The most striking results of the year are undoubtedly those of Köhler (3). Köhler started his investigations with an attempt to analyze the vibrations of the ear drum. In the delicate operation of attaching a small mirror to the surface of the drum for the purpose of reflecting and photographing light images which would be projected when the ear was stimulated by sounds, he had remarkable success. Although the tracings which he obtained on the photographic plate were too indefinite to be subjected to any strict analysis in accordance with Fourier's law, they were still exact enough to indicate certain general qualitative and quantitative differences.

Among the various movements recorded he was able to distinguish clearly those due to pulse, head movement and swallowing from those which were introduced by the regular vibrations of a sound-producing agency. Of these latter he differentiates two: (1) a sudden and continued alteration in the location of the curve, which he refers to the reflex of the tensor tympani, and (2) the propagating vibrations which are due to the oscillations of the hammer. From a study of the first factor he is able to set aside the so-called 'accommodation theory' with respect to the tensor tympani, and prove that the tetanic contraction, which sets in immediately a tone is sounded, is entirely dependent upon the intensity of the sound and has no discoverable relation to the pitch, or vibration rate. The use of the tensor is thus to be understood as Johannes Müller conceived it—a purely protective means against too intensive movements. It appears, however, to function for all sounds, and not merely for the more intensive stimuli. From the greater resultant tension which is indicated when two tones sound together than when either sounds separately, he concludes, further, that each tone in a combination loses something of its intensity, a fact which is borne out by the observations of hearing.

We must regret that the method has not thus far succeeded in producing curves of sufficient definiteness to determine the presence or absence of difference tones in the drum. Further investigations may possibly bring to light the facts in regard to this much disputed assumption of Helmholtz.

The most interesting results of this analysis are those bearing on the quality of tones. It was found that the fundamental forms of curves produced by the same instrument sounding at different pitches remained fairly constant, thus substantiating, in the main, Helmholtz's theory of quality as dependent on the number and intensity of overtones present. The perceptual quality, however,

depends more on the *relations* which remain constant among the overtones, than on the inherent qualities of the separate tones, as Stumpf has maintained, for the reason that the qualities of the single tones vary with their pitch. But before he had extended his investigations of the 'interval qualities,' the author stumbled upon certain phenomena which caused him to turn his attention during the remainder of this contribution to the study of the qualities of separate tones.

In investigating the forms of curves produced by singing the vowel *o*, he found that the relatively constant form which the curve revealed at one pitch level, became markedly different when the pitch was lowered, and indeed, the more different did it become, the lower the voice was pitched. The remainder of the investigation, dealing largely with the problem of vowel sounds, bring some rather surprising results in that they show quite conclusively that the whole range of pure tones possesses unmistakable vowel qualities. The progression is definitely in the order *u*, *o*, *a*, *e*, *i*, each vowel having its own characteristic pitch which it resembles more nearly than any other. The observations to prove this were made by various, for the most part unmusical, persons who were required to judge as to the resemblance to the vowels of a series of 30 tuning forks, which ranged from 163 to 4,000 vibrations, and were sounded behind them in irregular order. There were no deviations from the result above noted. All the pure vowels were clearly referred to definite regions of the scale with the necessary transitions intervening.

Further investigation of a more precise nature for the purpose of discovering the optimal pitch for each pure vowel, indicated conclusively that the vowels range in intervals of an octave above each other, and their vibration rates approximate those of the c's in music, at concert pitch. In other words, *u* has an optimal vibration rate approximating 264 vibrations; *o*, 528; *a*, 1,056; *e*, 2,112; *i*, 4,224. An octave above the *i* transforms that vowel into an *s* or *ch* sound, while an octave below the *u* reproduces the rumbling *m*; *ö* and *ü* are not pure tonal qualities and do not appear in the series, whereas *ä* is a transitional tone between *a* and *e*. The most striking thing in these results, obtained by four independent observers, is their almost negligible deviations from each other.

It is further shown by these results that the 'formant theory' of Hermann, who has assumed that a special sort of sound at a distinct region of the scale was the necessary adjunct of each vowel, is quite superfluous—as well as indemonstrable. The vowel quality is directly

given in the pitch, and without the presence of the particular pitch the vowel is impossible.

An interesting side light is thrown upon this investigation by the brief report of Ranjard (7) on the cure of the deaf by treatment with the 'vowel-siren' of Marage.[1] This instrument is constructed to reproduce the characteristic qualities of the vowels, which, according to Köhler, are their characteristic pitches. Ranjard's results, which show improvement in the condition of 75 per cent. of the cases treated, indicate somewhat diverse effects in different cases on the improvement for vocal sounds, music and noise. Some cases, he says, show improvement for words alone, others for vowels but not for consonants, and in one case he succeeded in developing only the hearing for noises. If we accept Köhler's results, those tests would seem to bear out the vast importance of the vowel qualities for hearing in general, rather than the necessity of assuming three different central functions for vocables, music and noise as Ranjard concludes.

Bingham in his Studies in Melody (1) has made a positive contribution to this problem by his critical and experimental examination of the theories of Lipps and Meyer on the subject. After differentiating the problem of tonal relationship from the more fundamental problem of consonance, he proceeds to evolve a theory of esthetic unity based on the phrase, period or totality of the musical sequence. The principal factors in this unity he finds to consist in (1) the preference for the 'falling inflection,' *i. e.*, the lower of two tones; (2) the 'law of return,' or desire for the tone started from, whatever it may be; (3) the preference for an ending which is expected, and which (4) is usually one of the tones of the tonic chord, preferably the tonic itself. The Lipps-Meyer 'law of 2,' or universal desire for an ending on the tonic, thus becomes only a special case in the general "tendency of every interval, yes, of even a single musical sound, to establish a tonality attitude."

On the further assumption that this attitude is essentially motor, the author proceeds to investigate the effect of hearing musical intervals on the rate of finger tappings. The results exhibit a general correlation between the beginning of a tonal sequence and retardation in tapping, also a fair correlation between the end of the sequence and acceleration in tapping. The more particular and to be expected correlations between related intervals and acceleration on the one

[1] The construction of this instrument is described very briefly in *C. r. acad. d. sci.*, 1900, 130, 746–748. The data are insufficient, however, to determine the exact pitch of the several vowels which it is capable of producing.

hand, and unrelated intervals with retardation on the other hand, are very imperfect. It is also to be noted that the experiments were performed upon only six observers, and were apparently not repeated. Thus the tapping rates given do not represent averages, but single experiments. Furthermore the source of the tones was a reed organ of presumably tempered intonation. From the above it is not very evident that so remote a coördination as is shown by the tapping finger is calculated to enlarge greatly our knowledge as to the nature of so characteristically psychic an attitude as that upon which our sense of melody depends.

Hornbostel (2) has given us some critical remarks of interest on the methods of comparative musical studies. He considers in some detail the results of (1) direct experimentation on non-European subjects, (2) tonal measurements made on their musical instruments, and (3) investigations on phonograph records. With respect to the first of these methods, it would appear that we are more often measuring the individual's capacity of attention in our tests than his real musical ability, since the important effects of practice are almost invariably lacking. Regarding the usage by Orientals of intervals smaller than the semi-tone, we must remember that these intervals are not often used as such, but usually in connection with larger intervals. What we must grant is the remarkable acuity for remembering a greater number of normal intervals than we use in our music. The examination of foreign musical instruments is greatly hampered by the necessity of keeping the instruments in tune, except in the case of pan's-pipes and the xylophone. Investigations on these instruments indicate a very high degree of differential sensitivity for the unusual range of intervals employed, since both in the case of Brazilian pan's-pipes and Siamese and Javanese xylophones the comparison of different instruments reveals a constancy of the intervals which can hardly be referred to mathematical knowledge or to the aid of physical instruments in their tuning. With respect to the melody problem we are told that we must not be prejudiced by our own current musical notions, but remember that primitive music is largely independent of harmony. It is thus the total form of the melody, not the mere tones or intervals, but the *motives* which involve changes in intensity, rhythm, relative duration, tempo and quality in the widest sense, which constitute the melodic significance of non-European music, —a conclusion to which Bingham would doubtless heartily subscribe.

Among the minor contributions of the year, the Peterson-Titchener controversy as to the seat of combination tones (**6, 10**), brings into

review most of the recent literature on the subject, but leaves the question still open as to whether the middle or inner ear is more largely responsible for these phenomena. Lasareff (5) has shown how the König wave-siren may be easily adjusted so as to produce phase differences in two simultaneously sounding tones, and states that these differences in phase have no appreciable effect upon the tonal sensation, thus contradicting König and supporting the view of Helmholtz. A simple device for measuring quantitatively the intensity of sound in auditoriums has been described by F. R. Watson (11). It consists essentially of a telephone receiver connected in series with a thermo-galvanometer. The device is portable and easily adjusted, and with the aid of resonators attached to the telephone receiver, appears to be well adapted for registering weak sounds.

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6. Peterson, J., Titchener, E. B. Do Combination Tones Originate in the Middle Ear? Psychological Bulletin, 1910, **7**, 169–173, 173–174.
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DISCUSSION

THE JAMES–LANGE THEORY OF THE EMOTIONS AND THE SENSATIONALISTIC ANALYSIS OF THINKING

Professor Titchener's latest discussion of the James-Lange theory of the emotions[1] seems, to the writer at least, to form an interesting parallel to some phases of the recent controversy over the analysis of thought processes. In his study of the emotions James "brought us back to the crude and raw of actual experience";[2] Titchener has given us[3] a description of the way in which an 'imaginal mind' does its thinking—an account unexcelled in clarity and vividness, which ranks with the James emotion chapters in power of stimulating the reader to careful detailed introspection. Yet James, in his emphasis on the organic factors in emotion, overlooked the elemental affective consciousness itself, and not a few psychologists to-day hold that Titchener and others,[4] in emphasizing the sensational constituents of thought, leave out of account its non-sensational constituents, its 'meaning aspect'[5] or 'relational elements.'[6] Further, several of Titchener's arguments[7] against the James-Lange theory of the emotions may be turned, almost as they stand, against the purely sensationalistic analysis of the thought processes. Such a turning of the argument against this later phase of sensationalism is the purpose of the present discussion.

On the negative side, Titchener points out (p. 481) the inconclusiveness of the argument that because I cannot experience or imagine an emotion without organic sensations, therefore the emotion must consist in organic sensations. Similarly, of course, as Calkins[6] and Angell[5] have already shown, a disproof (if such be possible!)

[1] *Text-book of Psychology*. 1910. Part II., pp. 474–484.

[2] *Ibid.*, p. 479.

[3] *Experimental Psychology of the Thought Processes*. 1909. Cf. esp. pp. 7–22; 184–186.

[4] Cf. esp. Colvin: A Marked Case of Mimetic Ideation. PSYCHOLOGICAL BULLETIN, 7, 1910, 260–268.

[5] ANGELL: Lecture delivered at Columbia University, Feb. 3, 1911; also *Psychology*, Ch. X., esp. pp. 245–249 (4th edition, 1910).

[6] CALKINS: Professor Titchener on the Thought Processes. PSYCHOLOGICAL BULLETIN, 7, 1910, 293–297; *Introduction to Psychology*, 1901, pp. 130 ff.; *A First Book in Psychology*, Ch. VIII., Appendix, Sec. VIII., 362 ff.

[7] *Text-book*, pp. 481–482.

of the existence of 'imageless thought' carries with it no disproof of 'meaning aspects' or 'relational elements' of consciousness as non-sensational. The writer, for one, never caught an 'imageless thought' in her own mind, but seems very clearly to herself to have caught non-sensational content along with the sensational, not only in 'thinking' proper, but also in imagining and perceiving.

On the positive side, Titchener gives two main arguments against the sensationalistic theory of the emotions. In the first place, he appeals to the testimony of introspection. A group of organic sensations is and remains just a group of organic sensations. Something more must be added to such a group to make of it a true emotion, and that something more is found in the affective elements of pleasantness and unpleasantness. In the same way introspection seems to many psychologists to show that a complex of images and sensations is and remains a complex of images and sensations; that something more—call it 'relational elements,' 'meaning aspect,' or what you will—must be added before it becomes 'meaning' or 'thought.'

In the second place, Titchener says, emotion and organic sensation are not the same thing, because the two vary, to some extent at least, independently. But the same is true, by Titchener's own showing, of images and 'meaning' in our thinking. Emotion and organic sensations vary independently of each other in their intensity, their quality, and their temporal relations.[8] With regard to the temporal rise and fall of meaning consciousness introspection is extremely difficult, and whether or not such meaning consciousness can be said to have an intensity is doubtful. But the consciousness of meaning certainly seems to have both clearness and quality, or kind, and to both of these attributes the variability argument may be applied.[9]

With regard to clearness, just as "there are a quiet joy and a boisterous joy," but "there is no evidence that the former is less intense than the latter,"[8] so for one person the sensational content of the "feeling of but" may be "a flashing picture of a bald crown . . . combined with an emphatic attitude,"[10] while for another it may be merely a vaguely localized fleeting kinesthetic image. That is to say, the sensational content for the first person is much clearer and more complete than for the second. Yet I think that even the sensationalists would hesitate to say that the meaning of 'but' is any less clear for the second person.

[8] *Text-book*, p. 482.

[9] CALKINS: PSYCHOLOGICAL BULLETIN, 7, pp. 296–297.

[10] TITCHENER: *Experimental Psychology of the Thought Processes*, p. 185.

Again, to apply the independent variability argument to quality, or kind, of meaning-content and sensation-content: for Titchener 'meaning' itself is "represented by the blue-gray tip of a kind of scoop . . . "; for one of his students it is the "mental unrolling of a white scroll"; for another "a horizontal line with two short verticals. . . ." [11] Must we then conclude that for each of these persons the consciousness of 'meaning' is an entirely different thing? It seems hardly possible, for, as Miss Calkins has pointed out,[12] they understand each other when they use the word. The logical conclusion is then, that the *kind of image* content is itself unimportant, so long as it 'represents' or 'carries' (I think the second of Titchener's terms a very exact expression of what seems to happen) the real meaning-content—in this case the relational consciousness of *within-ness*, which is common to all these different varieties of the consciousness of 'meaning.' In other words, just as Titchener concludes that "the sensations of the organic reaction cannot be identified with the emotion,[13] so the same kind of reasoning shows that the images which 'carry' the meaning-content cannot be identified with the meaning-content.

This conclusion is further corroborated by the fact that the independent variability of image-content and meaning-content holds not only for different persons, but for the same person at different times. If I may illustrate from my own case, I am, like Titchener, of a strongly imaginal 'mixed' type, with visual and kinesthetic imagery predominating. It is often, therefore, relatively easy for me to catch the sensation-image-content that 'carries' the meaning-content. But the puzzling thing, if the sensationalists are right, is that the image-content, when caught, turns out to vary so much from time to time, even when the meaning-content is the same. Take, for example, that same case of the consciousness of 'meaning' itself. Observations carried on during only a few days gave the following list of 'carriers' of the meaning consciousness:

1. A box-image suggestive of one mentioned by Titchener,[14] but more detailed, and visual as well as kinesthetic. In its most complete form a whitish box seen from above and a little to one side, with the lid half open showing a darkish interior. This visual image was sometimes relatively detailed, sometimes sketchy almost beyond recognition. Along with it there were more or less distinct kinesthetic

[11] *Ibid.*, p. 19.

[12] PSYCHOLOGICAL BULLETIN, 7, p. 296.

[13] TITCHENER: *Text-book*, p. 483.

[14] TITCHENER: *Psychology of the Thought Processes*, Appendix, p. 213, note 24.

images, either of bending over and peering in (images located in back of neck and eyes), or of lifting up the lid (an image in hand and arm). Sometimes one kind of kinesthetic imagery was present, sometimes the other. I never detected both on the same occasion.

2. A visual image of a pair of eyes looking into mine. Color and surroundings usually indefinite, but I seem to be looking into and beyond the eyes. Kinesthetic imagery or sensations such as usually accompany visual attention.

3. Visual image of word 'meaning.' Sometimes on a gauzy, half transparent background, again standing out in space, half concealing space behind. No kinesthetic imagery to be detected.

4. Visual image of eyes as in 2, but with the eyes looking toward my right. Kinesthetic tendency to turn my own head and eyes in same direction.

5. Visual verbal image, without the transparent background, but *with* a kinesthetic image (head and eyes) of looking away from word toward the right. The visual image often very sketchy and fleeting.

6. Sketchy visual image of sign-post or arrow, pointing away from me and toward the left. Kinesthetic image of looking in direction indicated.

7. A tactual-motor (often also auditory) verbal image. This occurred in combination with any one of the other six images, but even this could not be detected as invariably present. It often preceded and was replaced by one of the other images. On the two or three occasions when it occurred alone it 'carried' no 'meaning-content' at all, and I discovered that while saying the word over to myself my attention was really directed elsewhere.

Here are, then, six different sets of images as carriers of the meaning of 'meaning.' Must the meaning-content be different in all six cases? The answer of introspection is prompt and emphatic. 1, 2 and 3 all carry exactly the same meaning-content—meaning is that which is within something else. The relational consciousness of 'within-ness' is somehow present in each case, as it is in the examples which Titchener gives. In 4, 5 and 6, on the other hand, the meaning-content is somewhat different—meaning is that which is pointed at by something else. The relational part of this consciousness might perhaps be indicated by '*from x to y*,' where *x* and *y* are images which may themselves vary, but which always stand in the *from-to* relation. To these two different kinds of relational consciousness, the *within-ness* and the *from-to-ness*, correspond the two slightly different sig-

nifications which the word 'meaning' has for the observer. It will be noticed that whereas in 4, 5 and 6 the kinesthetic imagery, like the meaning-content, is practically the same, in 1, 2 and 3 there is no one kind of sense-content that is constant. It seems hardly possible, therefore, if this introspective record is correct, to hold that the meaning-content common to all three can be identical with any of the sensory-imaginal-content.

Examples might be multiplied. The above shows an instance in which a probably non-spatial relation or meaning is 'carried' in spatial terms. There are other cases where definitely spatial meaning-contents seem to include non-spatial relations. The verb 'to replace,' for example, means to me one of two things: it may mean to put an object back in the same place where it was; or it may mean to put an object in a place formerly occupied by another. The image-content of each of these two meanings is varied, depending largely on circumstances and especially on context. But the meaning-content is for each meaning constant. It includes, in Miss Calkins's terminology, the 'relational element' of *in-ness* (probably spatial), the temporal relations of *now* and *before*, and the non-spatial, non-temporal relations of *same* and *other*. In the first meaning of 'replace' it is the 'same' thing (image of whatever kind it may be) that is in the 'same' place (usually visual-kinesthetic image), but the 'in'-ness is 'other' than 'before.' For the second meaning of the word, it is an 'other' thing in the 'same' place as 'before.'

I do not wish to give the impression that all the images which carry the meaning of words and sentences are for me variable, for quite the contrary is the case. Not only have I a relatively fixed number-form, days-of-the-week form, and months-of-the-year form of which I make constant use, but the image-content of many words and phrases seems to vary from time to time only in clearness and detail, not in quality. Thus my own consciousness of 'but' is carried by a fleeting image of hand-movement closely connected with the explosive character of the tactual-motor-auditory image or sensation of the word. And my consciousness of 'essence' is carried by a visual image of a few dark layers or folds (of covering or drapery) to the right of and over (apparently partially covering) a pinkish blotch, accompanied by a distinctly unpleasant tactual-motor image which seems to be a remnant of shivering or shrinking. Whether the affective toning is due to the idea of 'bare essence,' or is a reminiscence of earlier struggles with Hegel's Logic, I do not know, At

any rate, practically the same sensational-imaginal-content occurs whenever I think of 'essence.' But I do not believe, in spite of that fact, that my consciousness of the *meaning* of 'essence' is any more definite or more stable than is my consciousness of the meaning of 'replace' or of 'meaning.' *Im Gegenteil!* Yet the image-content of 'meaning' is variable, while the image-content of 'essence' is constant. The testimony of my own introspection, then, would be that meaning-content cannot occur alone in consciousness without any sensational or imaginal content. Yet the two are not the same thing, but the meaning-content which is 'carried by' image-content is just that relational consciousness which introspection seems to find over and above, though intimately woven in with sensations and images.

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A CORRECTION

TO THE EDITOR OF THE PSYCHOLOGICAL BULLETIN:

Will you permit me to express my regret for two misprints in my first quotation from Titchener's *A Text-book of Psychology* on page 17 of the January number of the BULLETIN. The word 'plane' is printed for 'phase,' and asterisks are omitted before 'human experience.' Through a mistake—not that of either printer or publisher—the proof never reached me and therefore was not compared with the texts of the authors whom I quoted.

MARY WHITON CALKINS

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SPECIAL REVIEWS

QUESTIONED DOCUMENTS

Questioned Documents. ALBERT S. OSBORN, Examiner of Questioned Documents. With an Introduction by Professor JOHN H. WIGMORE. Two hundred illustrations. Rochester, N. Y.: The Lawyer's Coöperative Publishing Co. Large 8vo. Pp. 501.

The attention of psychologists should be directed to an important contribution along one of the many by-paths that now cross their route, and which one or another may be interested to follow when on professional service bent. The well-written, and equally well printed and embellished volume by Mr. Albert S. Osborn, dealing with the problems of genuineness involved in 'Questioned Documents,' is a valuable compendium of the data and methods of the study of handwriting for evidential purposes. This specialized art develops and establishes its technique; and to this Mr. Osborn adds original contributions, particularly in the application of photography to the revelation of obscure detail, which commonly was not intended to be revealed or revealing. His use in appropriate cases of the stereoscopic photograph as a convincing document is striking. The range of topics is very wide and makes one appreciate how large must be the base of the wedge that is directed pointedly to the precise deciphering of the most intimate of manual records. The fading of ink and aging of paper; the influence of the instrument and kind of pen or point; the changes of age; the vagaries of caprice; the variations of accident; the evidences of tracing, of erasing and alteration; the bold forgery that varies too far and the slavish one that holds too near the original; and again the refinements of the measuring apparatus; and the tricks of the trade and the rival ingenuity of the detective;—these are but a few of the phases that enter into the legal and technical aspects of handwriting, and are all admirably presented in this volume. It goes beyond handwriting in applying similar methods to so impersonal an instrument as the typewriter, which likewise has been called into question as yielding an evidential document. The make, age, pattern and individuality of the machine may be revealed by enlargements and analysis.

But apart from the utility of the volume to lawyers and others professionally concerned in establishing the genuineness or falsity

of disputed documents, the treatment carries direct significance to the study of handwriting as a psychological record. Handwriting thus becomes the most brilliant example of the subtle equilibrium between the delicate fluctuations of deliberately intentioned factors, and the subconscious accompaniments that shape the finer issues. We must intend, but not intend too much or too little. The stage laugh or the signalling or nervous cough reveal their unnatural origin through the same mechanism. But the visual record remains for the minute analysis that goes beyond impressionism, while yet it supports its verdicts. To execute naturally involves not too deliberate an intent or too emotional an interest, yet cannot dispense with these wholly. Yet again: handwriting as a document in individual psychology proves at once the subtle variations of which individuality is the complex issue (and incidentally confirms the difficulty of generalization and the limitations of a rough and ready impressionism) and the minuteness of differentiation that separates the genuine from the forgery. The graphological records, with their deadly parallelism of photographic enlargements, become quite as significant illustrations of the psychomotor mechanism as are the kymographic or automatographic records obtained in the laboratory in the pursuit of special problems. In one aspect they are more valuable, in that they are the issues of real motives and situations, which is an essential part of the complex of which they form the unwilling but enforced witnesses.

Such minutiæ of handwriting may be viewed as the data of a psychoanalysis developed pointedly in the interests of related problems; for here too the submerged mental procedures and affective and suppressed intents are to be objectively disclosed. Like all forms of such analysis, there is the difficulty of the technique and the uncertainty of the diagnosis and the interpretation. But these difficulties impart the fascination to the art, and serve to emphasize the bearing of the psychological principles involved. The interpretation of handwriting as a divining art is an old one. It was ambitiously exercised, not confined to the mere revelation of individuality, like the thumb print of modern days; but exploited to set forth the character of the writer and the ordinary range of his qualities. It was approached like palmistry or physiognomy as an art that was free to read character in line of palm or forehead or pen; and its practitioners were ready with final readings and convincing demonstrations. Yet for the advancement of knowledge such cultivation yielded little profit and a large crop of weeds. Transfer the interest, and refine

and concentrate the methods; and its returns though modest become secure. The older investigations of Preyer and the newer ones of Binet may be cited as illustrations of single aspects of the problem. The rationalization of the laboratory carries far beyond the limits of its *Arbeiten*.

As supplementary to the studies of the laboratory and approaching the problem from other interests and purposes, this study of the psychomotor mechanism as revealed in handwriting contributes a welcome illumination. Its value is due to the expertness of Mr. Osborn and the care and originality of his work. For a psychological library that aims to supply the study-material of related disciplines and arts, this volume is most suitable.

JOSEPH JASTROW.

UNIVERSITY OF WISCONSIN.

BOOKS RECEIVED DURING FEBRUARY

CLAPARÈDE, ED. *Psychologie de l'enfant et Pédagogie expérimentale.* Genève: Kündig, 1911. Pp. viii+471. Fr. 4.

HEYMANS, G. *Die Psychologie der Frauen.* Heidelberg: Winter, 1910. Pp. viii+308. Mk. 4.

INGEGNIEROS, JOSÉ. *Anales de psicología trabajos del ano 1909.* Buenos Aires: Etchepareborda, 1910. Pp. 353.

BERGSON, HENRI. *Matter and Memory.* Authorized translation by NANCY MARGARET PAUL & W. SCOTT PALMER. New York: Macmillan; London: Swan Sonnenschein, 1911. Pp. xx+339.

TELESII, BERNARDINI. *De Rerum Natura.* I. A cura de Vincenzo Spampanato. Modena: Formiggini, 1910. Pp. xxii+322.

DROMARD, GABRIEL. *Essai sur la sincérité.* Paris: Alcan, 1911. Pp. iv+243. Fr. 5.

FLOURNOY, TH. *Esprits et Médiums. Mélanges de métaphysique et de psychologie.* Genève: Kündig; Paris: Fischbacher, 1911. Pp. vi+561. Fr. 7.50.

ANONYMOUS. ("ELIZABETH MORISON and FRANCES LAMONT.") *An Adventure.* London & New York: Macmillan, 1911. Pp. vii +162. $1.25 net.

A Hand Book of the Millennium according to Oahspe the Cosmic Bible. Los Angeles: C. L. Brewer, 1911. Pp. 63.

KOSTYLEFF, N. *La crise de la psychologie expérimentale. Le présent et l'avenir.* Paris: Alcan, 1911. Pp. 176. Fr. 2.50.

MÉNARD, A. *Analyse et critique des principes de la psychologie de W. James.* Paris: Alcan, 1911. Pp. 467. Fr. 7.50.

BALDWIN, JAMES MARK. *The Individual and Society, or Psychology and Sociology.* Boston: R. G. Badger, 1911. Pp. 210. $1.50 net.

Memorias do Instituto Oswaldo Cruz. Ano 1910, Tomo II, Facic. 1. Rio de Janeiro: Manguinhos, 1910. Pp. 146.

CARUS, PAUL. *Truth on Trial. An Exposition of the Nature of Truth, Preceded by a Critique of Pragmatism and an Appreciation of its Leader.* Chicago: Open Court, 1910. Pp. 138. $1.

NOTES AND NEWS

Professor Irving King, of the University of Iowa, will give courses next summer in the University of Wisconsin on the 'social aspects of education' and on 'principles of education.'

Professor E. B. Titchener, of Cornell University, gave a course of lectures in February at the University of Iowa. While in the west he also lectured at the University of Kansas and the University of Indiana.

The following item is taken from the press:

It is announced in the *Revue scientifique* that Professor Hans Meyer has presented 150,000 marks to the University of Leipzig for the inauguration of an institute of experimental psychology.

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THE

PSYCHOLOGICAL BULLETIN

GENERAL REVIEWS AND SUMMARIES

THE FUNCTIONS OF THE CEREBRUM

BY SHEPHERD IVORY FRANZ

Government Hospital for the Insane, Washington, D. C.

For the understanding and for the proper evaluation of researches upon the nervous system, and especially of those on the cerebrum, it is necessary to have a knowledge of and to keep in mind the methods used for the collection of the facts which are recorded. The functional observations of the brain are derived from two general sources, the clinical and the experimental, which are not distinct but usually overlapping. The methods which have been used may be grouped under the following general headings: anatomy, physiology, and pathology. The anatomical studies have been conducted in a variety of ways: the myelogenetic, like those of Flechsig, by determining the development of different parts at different stages of anatomical and physiological development; the cyto- and myelo-architectonic, like those of Campbell and Brodmann, by which there is a finer differentiation of areas and which have led to certain physiological conclusions which will be considered below; and the general studies of human and comparative material. The physiological methods are experimental methods of stimulation and of extirpation, the former resting upon the assumption that the results of stimulation are similar or comparable to the normal working of the part, and the latter method resting on the assumption that if a part be destroyed there will be an absence of function corresponding to the normal function of that part. The general pathological methods are both anatomical and physiological; the former consisting largely of the examination of lesions and of degenerations from lesions, and the physiological, corresponding to the physiological methods of extirpation in animals, but, of course, dealing largely with human material.

With the exception of physiological extirpation all these methods have been used for the determination of function on the brain of man, but the greatest amount of knowledge of human cerebral functions has been derived from a combination of functional observations (clinical studies) and the anatomo-pathological observations of the brain after death.

The studies on man have certain advantages over similar studies on animals. The greatest advantage is the possibility of acquiring knowledge through the medium of speech of the effects of certain lesions and of stimulation, while the results from animals must be interpreted from less direct indications. On the other hand, it must not be supposed that all, and it must be admitted that very few, of the clinical studies have been the results of careful observations in a psychological sense. The possibility of careful psychological descriptions of the mental state accompanying cerebral lesions has seldom been realized, and it is the habit to record generalized deductions rather than minute observations on the mental side. Opposed to the possible advantages of the studies on man there is a decided disadvantage that we have always to deal with lesions which are naturally made and which most often extend to such dimensions that observations of small parts of the brain can not be made. The work on animals has this advantage; viz., that the lesions may be small and well defined but there is the disadvantage that the mental states of animals may differ largely from those of man.[1]

Although often the diversified methods have led to conflicting conclusions it is of interest to note that von Monakow (18, 19) largely from clinico-pathological studies, Bolton (3, 4) from histological and pathological investigations and Franz (9) from experimental work have recently brought forward somewhat similar hypotheses to explain the working of the brain in the complex mental processes. Each of these investigators show differences in standpoint, but all agree that the cerebral processes are much more complicated than has hitherto been supposed. We shall take up the hypothesis of von Monokow as the representative of these three and consider it briefly. Von Monakow believes that mental disturbances are produced by brain disturbances of a very varied character anatomically, but physiologically of the type of breaks or lowered conductivity of

[1] In a review of this character it is obviously impossible to consider all the articles, books, etc., dealing with the subject. The PSYCHOLOGICAL INDEX for 1909 contains about 90 titles upon the functions of the brain (including aphasia) and the bibliography contained therein is not complete. Although no accurate count has been made, the writer estimates that the total number of titles, including clinical articles, on this topic would not fall short of 200 each year.

the nerve elements which are active in the production of the mental phenomena. The partial or total lack of continuity of connection is called *diaschesis*. Diaschesis is an inhibition, "eine shockartige Funktionshemmung in primär nicht lädierten, vom Herd fern liegenden aber mit diesem anatomisch verbundenen Hirnstellen." According to this conception the effects of all kinds of brain lesions may be understood, for we have to look not for special centers but for connections. Very similar clinical phenomena may, therefore, be produced by anatomically distinct lesions, if the lesions be sufficiently small and definite.

In addition to the general view of cerebral conditions Bolton and Franz have considered the functions of the so-called association areas. Bolton believes that the posterior association area is concerned to a great extent with the language function, and the anterior association area with "the highest and latest evolved functions of voluntary attention and inhibition, and of selection and co-ordination of the various individually complex processes of cerebral association" (3). Although it is not expressly stated, it appears that the view of Bolton in regard to the frontal association area is similar to that of Franz, who considers the frontal area to be what may be called the efferent part of the association and the posterior association area to be the afferent part of the association.

These two authors do not apparently agree in regard to the possibility of a strict localization of sensation, of movement and of mental processes, for Bolton believes in a strict localization of an areal character. He goes even farther in this than most adherents of the localization school, and in this he is supported by those who, like himself, have investigated the structural arangements in the cortex. Bolton is, however, not as positive regarding the functional value of the histological studies as is Brodmann, for he says: "Beyond a relatively gross subdivision of the cerebral cortex into different areas, it is unlikely that the histological method will be of assistance, as cerebral function, even when relatively low in grade, consists of associational processes which involve many related regions of the cortex."

Brodmann (5) has given the most direct statement of a sort of phrenology from the histological standpoint. He believes in a strict histological differentiation of the brain into areas, and he asserts, but does not correlate with the histological findings, a functional differentiation and correspondence. In the consideration of this matter Brodmann uses the term 'physiologische Cortex-organologie'

to sum up his position. He has written: "Unsere Feststellungen rechtfertigen nicht nur im Princip die Annahme einer örtlichen Arbeitsteilung innerhalb der Grosshirnrindenfläche, sie beweisen auch mit unumstösslicher Gewissheit das Vorhandensein einer streng zirkumskripten regionalen Lokalisation gewisser Funktionen (p. 300) . . . die spezifische histologische Differenzierung von Rindenarealen beweist unwiderlegbar deren spezifische funktionelle Differenzierung . . . die linienscharf abgegrenzten und als besondere morphologische Organe aus der Rindenfläche heraustretenden Strukturzonen lassen sich nicht anders als durch die Annahme erklären, dass in ihnen ebenso scharf umgrenzte spezifische Funktionen lokalisiert sind" (p. 304). This is a return to Gall, if not to Spurzheim. It is not likely that such assertions without physiological facts to support them will pass unchallenged, and to the writer they seem far from proven, or even probable. This view may be taken as the extreme of deducing function from structure, but it is paralleled by the finer functional conclusions of Bolton, which, however, have some facts in support of them. I refer to the functional differentiation of the cortical layers. In regard to the layers Bolton (3) asserts that the first and fourth cortical layers, consisting mostly of fibers, may be omitted from consideration, but that the three other layers have different functions: the second, or outer pyramidal layer "subserves the psychic or associations functions of the cerebrum," the third layer "subserves the reception or immediate transformation of afferent impressions, whether these arrive directly from the lower sensory neurones or indirectly through other regions of the cerebrum," and the fifth layer "subserves the lower voluntary and instinctive activities of the animal economy, and thus forms a lower level basis for the carrying on of cerebral function." Here, apparently, Bolton passes from the extreme localization view to a more general view of the brain areas, in asserting that all areas have the same general functions, and it is difficult to make these two views cohere. Whatever may be our opinion of the hypotheses and conclusions already formulated from the cyto- and myelo-architectonic studies, it can not be gainsaid that this work is of great importance for the psychologist who has leanings towards the correlation of mental states and cerebral processes, and who takes a functional attitude.

These general matters will not be settled by hypothesizing or by strictly anatomical or histological studies. We must have functional observations to add to the anatomical facts, or we must have the anatomical observations to correlate with the physiological.

On the other hand, the functional observations must be made with as great care and with as great an amount of detail as the anatomical observations or we shall fail to bring about a correlation. One of the best examples of this is to be found in the studies of the relation of parts of the brain to speech functions. For a long time it satisfied most people to believe that the aphasias could be divided into the motor and the sensory, or into cortical, subcortical and transcortical, but because of the work of Marie and of von Monakow much doubt has been cast upon the validity of a strict anatomical localization and upon the discreteness of the supposedly different types of aphasia. We may pass over the matter which has caused much useless discussion, viz., the relation of aphasia to dementia or to mental deterioration, because that is to be settled only by definition, for the most important matter in connection with the discussion of speech and its disturbances is the consideration of the discreteness or the individuality of separate elements in speech. Physiologically, the understanding of vocal sounds, of gestures, of writing, etc., and the emission of movements expressive of bodily feelings and of ideas are to be understood only by taking the view that these are processes of association, and the disturbances of the ability to make movements expressive of ideas, or disturbances of the ability to understand are due to defects or faults in the association ability (physiologically considered) of the brain. The old doctrine dies hard, however, and there are many who still believe in a rather strict localization of visual images of words, auditory images of tones, kinesthetic images of vocal cord, tongue, etc., movements, which are supposed to be absent in certain aphasic conditions. (See 1, 2, 10, 11, 14, 15, 20.)

Although apraxia is an aphasic condition, often resulting from or coincident with lesions of the association fibers of the corpus callosum, and has been studied most by Liepmann, this author disregards the association explanation of Marie, and tends toward a view similar to the old classical localization (15). Beduschi (2) also cites cases apparently opposed to Marie's view, but with slight modifications in explanation all the cases may be brought into the category of association disturbances, and here the diaschesis hypothesis of von Monakow is most enlightening (17, 18).

Of association disturbances other than those connected with speech, it will be recalled that Franz, in 1902, found a loss of habits following the extirpation of the frontal lobes. This result has now been confirmed by the work of Kalischer (12), who has found losses of auditory-motor associations following the destruction of the frontal

areas. He has furthermore confirmed the earlier results in as far as the effects of the removal of one frontal are concerned, for he has also found that the removal of one frontal produces a disturbance of the association, but not an actual loss. In these experiments, however, the dogs which he used did not show an ability to relearn the habits which were lost after the removal of the frontals. From his work the conclusion is drawn that the frontal lobes are inhibition centers, because after the removal of the frontals the animals did not inhibit movements which they had been taught to inhibit. With equal value it might be said that the untrained dog, which retains its frontal lobes and does not inhibit instinctive actions, has no inhibition function. These results (of Franz and Kalischer) are the only ones which indicate clearly an association function for the frontal lobes in animals, and they are the only ones which indicate an association function for the brain of animals or man other than those connected with speech.

The histological differentiation of the brain into areas different from those into which the fissures divide it has led to newer work on the physiological differentiation and definition. The researches of Sherrington and Grünbaum antedated the histological differentiations, but resulted in a physiological differentiation of the motor area different from that which had previously been believed in. These authors placed the motor cortex anterior to the fissure of Rolando, and their results have been amply confirmed by the Vogts, by Lewandowsky and Simons (16), by Cushing and others. The extirpation of the precentral cortex is followed by paresis in animals, and the extirpation of the precentral cortex leaves the post-central cortex unstimulable (Lewandowsky and Simons). These results show that the cortex posterior to the Rolandic fissure has no fibers going directly to the muscles and that the movements which sometimes result from stimulation of this area are caused by the transfer of the stimulation or the association conduction of the stimulation to the neighboring motor region.

The article by Cushing (8) on the stimulation of the cortex in conscious patients is one of the most noteworthy contributions to the understanding of the brain which have appeared in recent years. It is not often that the brain in man is exposed so that during conscious waking states the results of manipulation and stimulation can be determined. Several cases of a character requiring surgical intervention, but in which it appeared inadvisable to use the ordinary methods of general anesthesia, submitted to the examination by

electrical methods of certain areas of the brain, and Cushing was able to demonstrate beyond doubt that the post-central convolutions are concerned with sensations from the skin and the underlying tissues.

Other sensory areas have not been investigated in such a manner in the brain of man, and for our information regarding the location and the extensity of these areas, we must depend upon the correlation of sensory defects (of vision, hearing, etc.) with the post mortem findings, and upon animal experimentation. The methods of training, first extensively used by Franz for the determination of the association functions, has been utilized by Kalischer, by Swift and by others for the determination of the extent of certain of the sensory areas of the cerebrum in animals and it appears to be settled that the temporal lobes are concerned with the perception—not only sensation—of sound. Swift (21) has shown that, corresponding with the results obtained by Franz in connection with the frontal lobes, after an animal has lost auditory-motor habits following the destruction of the temporal lobes it may relearn similar habits. The details of this work have not appeared and it is difficult to make a close comparison with the similar work of Kalischer of a few years ago. It appears, however, that the former work of Kalischer and the later work of Swift are in agreement. Kalischer (13) has, furthermore, investigated the functions of the occipital lobes by the training method, and his results are in accord with certain clinical observations, and with previous physiological experiments. Since this matter will be taken up for full consideration by the writer at an early date no further mention of it will be made here.

There remains space only for the consideration of the work of Graham Brown (6, 7), who has studied the effects of the removal of parts of the cortex on certain reflex functions. It has been shown that after a part of the cerebral cortex is removed the reflexes on the ipsilateral and those on the contralateral sides differ. The flexion excitability is increased upon the same side and the extension excitability is increased upon the crossed side, the scratch reflex may be abolished on the crossed side or the excitability of the reflex may be greatly lowered, while on the ipsilateral side there may be little change. These results indicate the extent of cortical control over functions which are often, but incorrectly, believed to be absolutely independent of the higher centers, and the work is to be considered in conjunction with that of certain Italian investigators, who found the reflex time to be increased upon stimulation of the frontal area.

Finally it may be said that the recent work on the histological differentiation of cerebral areas is most important for a definite understanding of functional localization, but it does not appear, either from the experimental or the clinical investigations, that the brain is a simple organ with simple but numerous functions, nor does it appear that efforts to bring about a revival of the doctrine of Spurzheim, even though it be based upon these anatomical studies, will help materially to advance the knowledge of this 'vehicle of mind.'

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THE COÖRDINATIVE MECHANISMS OF THE CENTRAL NERVOUS SYSTEM

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It has seemed worth while, in this first physiological number of THE BULLETIN, to present some of the leading results of one of the most illuminating books[1] on the functions of the nervous system that have appeared in years, although it is now nearly six years old; the more so because it has had no full review in the REVIEW publications and only scanty notice in most other journals. Nor is there much evidence that its results have sifted into general psychological literature. This may be due in part to Sherrington's condensed and difficult exposition. I shall attempt no full review but shall simply emphasize certain vital points concerned in nervous coördination, or integration. Curtailed space prevents my discussion of the later results of Sherrington and others, many of which will be found, however, in Woodworth's article in this number.

The unit mechanism in nervous coördination is the reflex arc. Two clear fields of investigation therefore open out: (I.) the coördinating activity of the simple arc, and (II.) the type of coördination effected by the compounding of such arcs.

I. *Coördination in the Simple Arc.*—The main function of the receptor is "to lower the threshold of excitability of the arc for one kind of stimulus, and to heighten it for all others" (p. 12). Its chief contribution to reflex mechanism is, therefore, *selective excitability*, by which, to a degree, confusion among reactions is forestalled.

The contributions of the reflex-arc to nervous integration rest, however, chiefly on the properties of nervous conduction in the arc as distinguished from those in nerve-trunk. The full list may be found on p. 14. Conduction in reflex-arcs exhibits, among the others, these eight characteristics: (1) "slower speed as measured by the latent

[1] SHERRINGTON, C. S. *The Integrative Action of the Nervous System.* New York: Scribner, 1906. Pp. xvi + 411.

period between application of stimulus and appearance of end-effect, this difference being greater for weak stimuli than for strong." In nerve-trunk any delay is due wholly to propagation time, nor is there good evidence that the speed of conduction is less with strong stimuli than with weak. This *identity* of speed in nerve-trunk from strong and weak stimuli alike and the great *difference* in speed in reflex-arc from strong or weak stimuli respectively is important (*e. g.*, it strengthens the argument that the delay is due to some structure other than nerve fiber proper; it enables an organism to respond with varying degrees of rapidity to stimuli of varying degrees of intensity). Sherrington then discusses the question whether the latent time is consumed in 'setting' or establishing a connection between neurones in an arc, the current thenceforth flowing perhaps as fast as in nerve-trunk, or whether it is inherent in the process of arc-transmission. He inclines towards the latter position, since if a flexion reflex of the hind limb of a spinal dog be elicited and then, while flexion is still in progress, a stronger stimulus be suddenly introduced, *there occurs only a negligible reduction of the latent time for this incremental reflex from that of the initial reflex.* It appears, therefore, that the latent time is due to a process the same in nature, whether in initiating a reflex from a resting arc, in increasing a reflex already in submiximal activity, or, probably, in maintaining a reflex in unaltered continuance. These considerations argue against any supposition of amœboid movements as controllers of conduction between neurones and, since even while at rest the arc is as well prepared for immediate transmission of impulses as when actually engaged in activity in the very direction the new impulses would require, they argue a greater need for active inhibition in the coördination of arcs that employ a final common path but for different purposes. (2) "Less close correspondence between the moment of cessation of stimulus and the moment of cessation of end-effect, *i. e.*, there is a marked 'after-discharge.'" The phenomena of latent period and of after-discharge enable us to characterize conduction in arcs as exhibiting inertia and momentum. After-discharge is an analogue to the visual positive after-image. (3) "Less close correspondence between rhythm of stimulus and rhythm of end-effect." Sherrington's work on this well known principle shows that (as is also true for the latent period) different reflexes have different rhythms developed, doubtless, to meet the peculiar adaptive purpose of the given reflex. (4) "Less close correspondence between the grading of intensity of the stimulus and the grading of intensity of the end-effect." Such grading is,

however, found in arcs. The 'all-or-nothing' principle does not there hold. The increase in the reflex effect is in power and in amplitude; the rhythm changes little. This adaptability of power and amplitude, like that of speed, to intensity of stimulus, is manifestly biologically valuable in many reflexes. As to the problem whether the increase in response to graded stimuli is due to more muscle fibers being involved or to a greater intensity of nervous impulse in those already active, Sherrington leans to the latter alternative, because the mechanism of a reflex is, although anatomically a collection of units, itself a unit, and discharges as a unit. Even the phenomenon of nervous irradiation supports this view, since the irradiative progress to new muscle is sudden—a progress from unit center to unit center. (5) "Considerable resistance to passage of a single nerve-impulse, but a resistance easily forced by a succession of impulses (temporal summation)." (6) "Irreversibility of direction instead of reversibility as in nerve-trunks." (7) "Fatigability in contrast with the comparative unfatigability of nerve-trunks." (8) "Refractory period, 'bahnung,' inhibition, and shock, in degrees unknown for nerve-trunks." *Refractory Period:* The phenomenon of rhythm in nerve-cell discharge is itself presumptive evidence of a refractory phase. In reactions of the central nervous system it is "a state during which, apart from fatigue, the mechanism shows less than its full excitability" (p. 45). In the scratch-reflex, for instance, flexion and extension each occurs at a rhythm of from 4 to 6 per second. This rhythm is *independent of the rate of stimulation and remains unaltered under increased intensity of stimulation.* It appears, therefore, that when after each contraction of the flexor concerned in scratching the extensor phase of the reflex supervenes, no intensity of stimulation can excite the now inhibited or refractory flexors to activity, and the same may be said of the extensors when the flexors are active. The flexors and the extensors exhibit, that is, a rhythmic alternate 'refractory phase.' It is apparent that in cyclic actions occurring in rhythmic series (scratching, swallowing, blinking, rhythmic stepping of the limbs, etc.) the phenomenon of refractory phase contributes immensely to the coördinated result.

Even from this enumeration of the characteristic differences between nerve-trunk and reflex-arc conduction it is evident that many of the constant as well as the variable *integrative* features of the 'simple' arc are determined by its own conductive peculiarities. Different reflexes, too, to an enormously greater extent than do different nerve-trunks, exhibit, inter se, differing peculiarities of con-

duction (*e. g.*, different latent periods, rhythms, refractory periods, and varying degrees of grading of response to graded stimuli); their individual *conductive* idiosyncrasies obviously determine, in part, at least, their individual *coördinative* or *integrative* differences developed in biological adaptation.

As to the physiological loci of most or all of the foregoing differentiæ of reflex-arc conduction, the synaptic surfaces of separation between neurones, in the gray matter, appear the most probable, especially since less and less influence on conduction is being attributed to the nerve-cell body. As an example of the type of Sherrington's reasoning, in seeking the locus of the phenomena observed, let the following, concerning the refractory phase, suffice. The 'scratch-reflex' is elicited by stimulating any point or points within a large saddle-shaped field of skin on the back and sides of the spinal dog. The mechanism of this reflex consists probably of an afferent neurone (or a bundle of them), a long descending proprio-spinal neurone (determined by tracing the course of degeneration), and a motor neurone. At least two synpases are, therefore, involved. Now the seat of the refractory phase can be neither in the motor neurone (or final common path) nor in the end plates in the muscle, since, when excited directly or through nerve-trunk, muscle exhibits no refractory phase nor, furthermore, is it present when the motor neurone is used by other reflexes (the flexion reflex) or excited to contraction through the pyramidal neurones from the cortex. It does not lie in the skin nor in the afferent neurones, since, when the reflex is in progress from an excited point *A*, the excitation of another point *B*, within the stimulus area, does not alter its rhythm nor complicate it in any way. This fact points to a part of the reflex mechanism common to impulses from both *A* and *B*. This cannot be, however, any nexus between the afferents, since we have no knowledge of a collateral connection between afferent neurones. The refractory phase may be referred, therefore, with great probability, to the synaptic connections between the proprio-spinal neurone mentioned and the afferent paths *A* and *B*, through collaterals from their ascending-descending branches. It must at least be *central* to the afferent neurone and the motor neurone concerned.

II. *Coördination between Simple Reflexes—the Compounding of Reflexes.*—A natural transition to the consideration of complexer reflex-patterns lies through the phenomena of 'inhibition.' The refractory phase in the single arc is itself a form of inhibition. If, further, the flexion-reflex be evoked in the hind leg of the spinal dog

by stimulating, say, the foot, it is found even by direct palpation, that the knee extensors of the same knee are actively inhibited, even as to their tonus; complete relaxation ensues. This is typical of any opposed muscle groups at the same joint, inhibition in one group of muscles being the *simultaneous* counterpart to excitation in the opposite group. Any such flexion involves, therefore, two simultaneously acting reflexes—perhaps the simplest type of coördination between simple arcs. The *same afferent impulse* utilizes two opposed arcs to opposite effect. The seat of the duplex process is intra-spinal and doubtless synaptic. Nor can any mere increase in intensity convert the inhibitory effect into an excitatory; further, when one reflex is employing given muscles, other reflexes are normally inhibited from utilizing them. These two facts clearly show that the great biological contribution of inhibition to the organism is to fortify momentarily dominating reflexes in the possession of their arcs until some shift in the environmental constellation of stimuli brings to orderly prepotence further adaptive reflex acts.

Inhibition is thus one of the chief mechanisms of integration. Another is 'alliance.' Properly to understand them it is, however, first necessary to refer to what Sherrington calls the principle of *the final common path.* The various receptive neurones are so many private paths, each sacred to its own adequate stimulus. Any given private receptive path may, however, eventually play on a great number of different effector organs and, conversely, many such private paths may converge on one effector. The motor neurone leading to the effector, becomes thus a 'final common path,' a public path, receiving impulses from many afferent arcs, each using it, perhaps, to slightly different purpose. Certain consequences of this principle are (1) the essential qualitative identity between impulses arising in different afferent nerves, (2) the capacity of the final common path to respond to all the different rhythms of the different reflexes employing it—its aperiodicity, and (3) the necessity of successive and not simultaneous use of this path. Nervous coordination reduces largely, therefore, to a competition among various reflex arcs for the possession of the final common path. Inhibition is one of the determinants of the outcome. Another is 'reflex alliance.'

'Alliance' means that various reflex arcs reinforce one another's action on one or more final common paths. If points *A* and *B*, as much as 10 cm. apart, in the receptive area of the scratch reflex, be stimulated, each subliminally, the reflex occurs. If the stimuli are

supraliminal it will be increased. This is not primarily due to the excitation of different groups of fibers in the final common path,[1] but to the *intensive augmentation of the same fibers*, since the scratch, initiated by *A*, does not alter in rhythm when *B* alternates with *A*. Impulses from all points of the receptive field embouch, therefore, on the *same* final common fibers: the mechanism is a unit, composed of many allied reflexes. A more extensive type of alliance is seen in the mutual reinforcement between extero-ceptive (external stimuli) and proprio-ceptive (internal stimuli, *e. g.*, kinæsthetic) sensitive fields, typified by many reflexes, involving, first, a response to an external stimulus, second, the consequent excitation of sensory organs in the contracting muscles and, third, an alliance between the two sets of arcs, both acting on the same final common path. There are, however, still wider alliances than these, namely, those involving arcs from widely scattered sensory points. If, for instance, the various digits of the hind foot of a bulbo-spinal dog are stimulated, they mutually reinforce one another's action on the final common path of the flexion reflex: to these there may ally themselves impulses from the crossed fore foot, the tail, the crossed pinna, and, doubtless, in the intact animal, from the otic labyrinth, the olfactory organs and the eyes. Furthermore, just as reflexes may be allied in exciting a muscle they may be allied in inhibiting it. And the inhibitory alliances may be either simultaneous or successive. Whenever, for instance, a simultaneous alliance is in progress, *an inhibitory alliance of all the arcs to the contrary muscle groups, relaxing them, is involved;* whenever one group of reflexes dispossesses another from the use of the final common path we have successive inhibition or 'interference' and, finally, when "the threshold of each succeeding reflex is lowered by the excitation just preceding its own" (p. 184), we have successive alliance. A given reflex usually lowers the threshold for succeeding allied arcs but it may also lower it for antagonistic arcs: in the stepping reflex of the dog—and this is doubtless typical of most 'alternating' reflexes—an antecedent reflex not only brings about an ensuing stimulus for the opposite movement, but predisposes the opposite centers to activity. Such alliance between mutually inhibitory successive reflexes obviously favors the orderly prepotence of the successive units of a biologically valuable serial act. This coördinative interplay of successive reflexes is nicely seen, too, in the so-called 'compensatory reflex'—"a return to a state of reflex equilibrium which had been disturbed by an intercurrent reflex to which the compensatory reflex is the diametrical antagonist"

[1]See above, differences between reflex-arc and nerve-trunk conduction, (4).

(p. 203). In the state of 'decerebrate rigidity' of the muscles the flexion reflex breaks in on a strong reflex tonus (active extension) and the reflex itself induces supervening tonus, stronger, often, than before, probably largely through the afferent nerves of joints, muscles, etc. These proprio-ceptive nerves, together with those of the otic labyrinth are, indeed, probably responsible for the tonus of the skeletal muscles—itself a reflex and involving always coincident tonal inhibition of muscles antagonistic to those in tonus.

One important matter remains. Manifestly the orderly sequence of *reactions* in an organism is not determined by any orderly sequence of *stimuli*. The organism is, rather, always exposed to a jumbled constellation of stimuli, shifting from moment to moment, some of which secure at any instant prepotence over the rest, to be then displaced by others. What factors determine, chiefly, the order of prepotence among the arcs competing for possession of the final common path? One is *spinal induction;* a given stimulus, by central spread, lowers the threshold for allied arcs and, as we have seen, for the arcs next to supervene, *e. g.*, in 'alternating' and in 'compensatory' reflexes. Secondly, *fatigue* loosens the hold of a reflex on the final common path, assisting, thus, to serial variety of reflexes; for the final common path is itself relatively indefatigable—the seat of the fatigue being, doubtless, intraspinal. Third, the *intensity* of reaction of the afferent part of an arc, relatively to its rivals, is perhaps the most powerful influence determining prepotence. Both intensity of stimulus and degree of excitability of the afferent arc are, of course, here concerned. Fourth, the *functional species* of a reflex contributes to determine prepotence. In the cord noci-receptive reflexes—those usually elicited by harmful stimuli, or stimuli arousing pain in an intact organism—tend to displace other reflexes. Examples are the flexion-reflex (injury to foot) and the scratch-reflex (bite of flea). Further, however, any reflexes evoking strong affective accompaniments in the intact animal, are likely to be prepotent, *i. e.*, the sexual. These types of reflexes stand, thus, at the upper end of the prepotency scale—the most difficult to dispossess—while at the lower end are the tonus reflexes that maintain habitual attitudes and postures. Finally, all four of these factors determining reflex prepotence are reducible to intensity of reaction.

REFLEX ACTION

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The past two years have witnessed several interesting developments in the knowledge of reflex action. Sherrington's 'decapitate preparation' (**9, 6**) affords a convenient means of studying and demonstrating the functions of the mammalian cord. After ligaturing the carotid and vertebral arteries, and, indeed, the whole neck with the exception of the trachea, he simply beheads the animal; after which the anesthetic can be dispensed with, and the animal kept alive for several hours by artificial respiration combined with sufficient warmth. On such a preparation, Sherrington (**8**) has examined the theoretically important scratch reflex in the cat, and finds that the rhythm of the scratching movements originates in the cord and not in the sense organs, since it appears on exciting the cord itself.

What may be called 'switching reflexes' are brought to light by the observations of Magnus (**3**). The reflex movement elicited from the leg of a spinal dog differs according to the posture of the leg. The same stimulus which gives flexion from a position of extension gives extension if the limb is already flexed; and it is possible for the experimenter to switch the movement back and forth from extension to flexion by simply altering the initial position of the limb. These effects are not obtained if all of the sensory fibers of the limb are severed by section of the dorsal roots, but they are not destroyed by injections of cocaine, stovaine, etc., beneath the skin or into the joint capsules. The afferent fibers concerned are therefore those from the muscles; the 'switching' is a function of the proprioceptive system. Afferent impulses from the muscles so set the switches in the cord as to determine the reflex path of other impulses.

Similar instances of switching are shown by Magnus (**4**) in case of the scratch reflex. When a spinal dog is standing, irritation of the skin causes the leg of the same side to scratch, while the leg of the opposite side is extended and abducted as if to support the body. If, however, the animal is laid on its back, the scratching may be bilateral; but either leg can be prevented from participating in the rhythmic movement by holding it gently in a position of extension and abduction. This also is a proprioceptive effect. But similar effects can also be produced by cutaneous or subcutaneous stimuli; for if the animal is laid on one side, or if one side is pressed against, it is always the opposite leg that scratches, on whichever side the

stimulus to scratching is applied. Thus both pressure on one flank and extension and abduction of one leg tend to switch the scratching movement to the opposite leg; if these two influences act counter to each other, pressure on the flank yields to the posture of the limb, and the latter influence controls. We get here some insight into the complicated spinal mechanism governing the selection of reflexes.

Further examples of proprioceptive reflexes are brought out in Sherrington's recent work (7). In certain conditions (decerebrate rigidity especially, but also in spinal preparations), when a muscle which is in tonic contraction is forcibly stretched, it yields to the stretching force, gives up part of its contraction, and assumes a new length or degree of contraction, maintaining this after the stretching force has ceased to operate. This is the 'lengthening reaction'; and an opposed 'shortening reaction' can be induced, in the same conditions, by shortening the muscle. The muscle is plastic; it retains, by its own contraction, the length which has been impressed on it. This it no longer does if its afferent nerves are severed, though it needs no other afferent nerves than its own. Not only its tonus in general, but the plasticity of its tonus, is dependent on a reflex arc starting in its own sense organs, and leading through the cord back to itself. These strictly local reflexes are probably importamt in adding duration and steadiness to movements induced by other stimuli, in maintaining postures, and in modifying reflex and perhaps voluntary movements in accordance with the resistance encountered.

The relations of several important reflexes—the flexion of a limb and the extension of the opposite limb, the stepping reflex, and standing and walking—are subjected to minute analysis by Sherrington (10). The simple flexion reflex, by which the leg is withdrawn from a noxious stimulus, involves the contraction of certain muscles and the relaxation of others; and the same muscles are involved whatever be the intensity of the movement. Increasing the force of the response does not mean the bringing into play of new muscles, but a more complete excitation or inhibition of the same muscles. The reflex may however spread to the opposite leg, inducing there, usually, an extension. The 'stepping reflex' of the spinal animal comprises alternate flexion and extension of the hind limbs; and the flexion is almost identical with the simple flexion reflex, and the extension with the extension reflex. The main peculiarity of the stepping reflex is its rhythmically alternating character. It consists in an alternation of antagonistic reflexes, though it may be aroused

and maintained by a continuous stimulus. Several factors probably combine to produce the alternation: a proprioceptive reflex from each muscle excited, leading to the inhibition of that muscle and the excitation of antagonists; a 'switching' of the original stimulus, such as was noted by Magnus; and a purely central rebound effect, by which the spinal mechanisms inhibited in each phase of the stepping tend of themselves to become active the next instant. *Standing* is only imperfectly executed in a purely spinal animal, but becomes fairly efficient when the midbrain and everything below is left intact. It consists essentially in a tonic excitation of extensor muscles, with inhibition of the flexors; and this reflex is of the proprioceptive order, depending, for each extensor muscle, on afferents from that same muscle. The plastic nature of this extensor tonus permits the standing posture to vary according to the conditions. *Walking*, which also occurs in decerebrate animals, is a combination of the standing and the stepping reflexes; the stepping does not altogether inhibit the composite extensor tonus, but alternately weakens and reinforces it in different muscles and in due sequence.

Brooks (1) contributes to the study of the perplexing problem of 'spinal shock,' first by introducing quantitative methods (measurement of the stimulus needed to arouse a reflex), by which means he is able to demonstrate minor degrees of shock in animals which do not show the phenomenon to ordinary observation; and second, by showing the existence of shock after cutting the dorsal roots as well as after transection of the cord. He also finds that those species which show severe shock after spinal transection show it also after cutting the dorsal roots, whereas those which show little loss of reflex excitability after the former operation show little also after the latter. The results favor the interpretation of shock as a loss of spinal tone due to the cutting off of normal excitants, rather than as a stoppage of reflexes due to injury of the paths directly involved.

Porter (5) applies quantitative methods in the endeavor to penetrate the secret workings of the vasomotor center. He argues that, if the bulbar cells take an active and directive part in vasomotor reflexes, their influence should be visible in distorting the statistical distribution of the reflex effects; whereas the variation of the reflex blood pressure, under uniform conditions, shows a 'normal' or symmetrical distribution, free from any sign of a distorting influence. Moreover, the curve of relation between intensity of stimulus and energy of response shows the same form as in direct excitation of a muscle. He thinks it probable, accordingly, that the bulbar center

does not actively 'govern' or 'control' or 'preside over' the vasomotor system, and calls attention to the unverified assumptions underlying these figurative ways of describing the function of nerve centers.

Is the knee jerk a true reflex? Snyder (11) and Dodge (2) answer in the affirmative. The latency of the action current of the muscle, as determined by Snyder, varies from 8 to 15σ, with an average of 11σ, when the tendon is struck, but is much less (2.5 to 3.6σ) when the muscle is excited by an electric shock. The delay of the jerk is thus too great to be explained as a latency of direct muscular response. On the other hand, if Piper's recent determination of the velocity of the nerve impulse (120 meters per second) be accepted, and Snyder has verified it, the time necessary for a reflex from the knee to the quadriceps may be as little as 15–18σ, and thus not far from the observed latency of the knee jerk.

Dodge also calculates the reflex time to be in close agreement with the observed latency of the knee jerk; and in addition he adduces several other lines of evidence, among which the most striking is perhaps the fact that the contraction of the muscle does not start at the point of application of the blow, but, if anything, at the opposite or proximal extreme of the muscle. Dodge's work contains a critical study of the technique of registering the knee jerk. He adopts a record of the thickening of the muscle, by mechanical or, even better, by photic transmission, as the form of record most free from errors. He examines the dependence of the latency and force of reaction on the force of the blow; and, by stimulating with a rapid series of blows, he finds the knee jerk to have a refractory period of about one tenth second. Many other results of his valuable study must here be passed by for lack of space.

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THE PRESENT STATUS OF THE NEURONE THEORY

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There are four main theories of the development of the nervous system which are held with certain modifications by the various investigators.

They may be enumerated as follows: (1) The von Baer-Hensen or intercellular bridge theory, which is defended in a modified form by Hans Held. (2) The Schwann-Balfour or cell chain theory. (3) Hertwig's theory. (4) The His-Cajal-Waldeyer or the neuroblastic outgrowth theory.

The von Baer-Hensen theory may be briefly stated as follows: that as a result of incomplete cell division the various cells are connected by protoplasmic bridges, and by a process of local differentiation in the cells and their protoplasmic connections, the nerves are formed.

This has been modified by Held (4) who accepts as true the protoplasmic connections or plasmodesms, but claims that the nerves are formed not by local differentiations but by the outgrowth of neurofibrils from the neuroblasts.

These fibrils follow the preformed protoplasmic pathway which not only guides them to their end organs but assists, Held assumes, in some unexplained way in the process of differentiation.

The Schwann-Balfour or cell chain theory essentially assumes that a nerve is developed by a series of cells joined end to end which thus forms a path from the central nervous system to the end organ. At present the most notable exponents of this theory are O. Schultze and Bethe, the latter holding the theory in a modified form. The cells of Schwann of the neurilemma are the cells which are supposed, by end to end junction, to form the nerve. These have migrated chiefly from the ganglia along the dorsal roots or, in some cases, partly from the wall of the neural canal along the ventral roots. Harrison's experiments however on the developing nerves of the tadpole have shown that even when no cells of Schwann were present the nerve

fibers developed as usual. Also his observations on the growth of the axones of tadpole neuroblasts in frog lymph, and similar observations by Burrows in the case of the chick (which will be referred to later) make it impossible longer to hold this theory. Held has also called attention to the fact that the lack of nuclei in the region of the outgrowing axone militates against the cell chain theory.

Hertwig's theory, like Hensen's, assumes that the cells of the central nervous system are connected with the peripheral end organs by the intercellular protoplasmic connections, but while Hensen considers this a primary connection, the Hertwigs look upon it as a secondary one.

The fourth or the His-Cajal-Waldeyer theory is the one more generally held, having among many others as supporters, Retzius, Lenhossék and Harrison. This theory was originated by Kupfer but especially developed by His. It essentially holds that nerve fibers and dendrites are outgrowths of certain embryonic neural cells, the neuroblasts, which by growing to the periphery, connect the end organ with the nerve centers: that these cells or neurones constitute genetic, nutritive and functional units, and that their processes lie closely applied, but never or perhaps rarely are fused as a secondary concrescence.

Gerlach's nerve network in the gray matter between the cells is not a network of anastomosing fibrils, but rather a plexus of intertwisted fibrils. The outgrowing axones pass through the intercellular spaces, and not through the protoplasmic cell connections as is postulated by the Hensen-Held theory.

In a consideration of the neurone theory it is essential that we have clearly in mind what we mean by that term, as often considerable confusion has arisen in the discussion of the theory on account of the different interpretations and use of the term, resulting in needless conflicts and retarding the search for truth.

The essential fact of the neurone theory is that a nerve cell, and its processes, axones and dendrites, are developed from a single formative center, the neuroblast. These may be primarily or secondarily connected together by protoplasmic processes forming a syncytium but the perinuclear protoplasm constitutes a formative center and the nucleus controls the synthetic metabolism. The neurone is in a certain way, therefore, the functional as well as the structural unit. If the cell body and nucleus are the genetic and formative centers, it matters not if some of the processes may primarily be united as a result of a partial cell division, or secondarily form by

fusion with other cells, a syncytium. The cell evidently is not always the discrete unit we formerly considered it.

The evidence in regard to the genesis and minute structure of the nervous system is derived from the following sources:—histology, embryology, experimental pathology and experimental embryology.

The increase in our knowledge of the histology of the nervous system has been largely dependent upon improvements in the microscope; and in devising new micro-chemical methods which will show finer structural differentiations. So we find that the invention of the chrom-silver methods as used by Golgi and Cajal, the methylene blue and gold chloride methods, and finally the more recent technique of staining neurofibrils, have constantly advanced our knowledge of the nerve cells and their relations.

The difficulty here is that we are dealing with such minute objects, and so closely related, that it is almost impossible to tell, for instance, whether fine fibers are fused or only lie closely applied; so that it is possible for skilled observers to interpret the same appearances very differently. Also, micro-chemical methods sometimes stain partially, or imperfectly, failing to show at all times fine details completely. These various reasons partly explain the diversity of opinion, especially in regard to details of structure.

Recent histological work has added but little to our knowledge of the nervous system, but one of the most noteworthy contributions is the work of Hans Held (4). Here he not only explains his own investigations, but considers critically the bearing of various other investigations on our knowledge of the genesis and finer structure of the nervous system. Held discusses the process of neurofibrillation, the development of the motor and sensory nerves and of the sympathetic system, the histogenesis of nerve and glia cells, and the nature of nerve formation. He studied material from various animals representing the different classes of vertebrates including the Leptocardia and Cyclostomata. Briefly considered he finds that the process of neurofibrillation is much the same in the various classes of vertebrates, starting near the nucleus and on one side, in the fibrillogenous zone, forming a net-work and extending out into the plasmodesms. In the motor nerves the process starts in the neuroblasts in the neural wall, and extends through the intercellular bridges to the myotomes, and in the sensory spinal nerve neurofibrillation begins in the cells of the dorsal ganglia which have previously migrated from the neural crest.

In certain sensory cranial nerves neurofibrillation begins both

in the ganglia and in the groups of ectoderm cells of the branchial region. In both spinal and cranial nerves and in all classes of vertebrates the neurofibrils extend from the neuroblasts through the protoplasmic bridges to the end organs. That the neurofibrils may grow through the cells of the neural wall, as Held claims, seems evident from his plates. This has also been described by Harrison (3). Held's figures seem to uphold his assumption that the neurofibrils also grow through the mesodermal plasmodesms. The probability is that the outgrowing axones pursue a fairly direct course to their end organs, passing through both the intercellular spaces and cells also, if they are in the way. Whether the guiding force is of the nature of chemotaxis or some other tropism is not known.

Held assumes that if the neurofibrils grow out from a neuroblast and pass through the protoplasmic intercellular connections, this negatives the neurone theory. According to our understanding of the neurone theory this is not the case. So long as the process grows, or neurofibrillation extends as a result of the formative energy of the nucleus and perinuclear protoplasm of the neuroblast, it makes little difference where the outgrowing process goes, whether through intercellular spaces or cell protoplasmic connections.

Held's assumption that the protoplasmic processes in some unexplained way are necessary for the growth of the axone and the neurofibrillar differentiation is, we believe, not well grounded. If it be true that the neurofibrils in the cell bodies of two neuroblasts fuse together and extend out into processes as Held has described in his poly-neuroblastic formation, this would constitute a serious menace to the neurone theory. It seems to the writer, however, that the evidence for this is not satisfactory, and that Held's findings from his study of neurofibrillation and the histogenesis of the nervous system in the various vertebrates are not such as to negative the neurone theory.

Kato (5) agrees with Held that the neurofibrils form a network in the cell, especially about the nucleus. Even fibrils in the processes divide and unite with one another. Whether such networks exist or the fibrils are free and separate in the cells and processes, is of no importance so far as the truth of the neurone theory is concerned. Van de Velde (9) has described the terminal neurofibrils as penetrating the cells of Grandry's corpusles. This is contrary to the work and the general opinion of most anatomists, which is to the effect that nerves terminate peripherally in fine fibrils on the epithelial cells, but do not penetrate them. So far as the truth of the neurone theory

is concerned, however, it matters not whether the peripheral nerve terminations are intra- or extra-epithelial.

It seems probable that we have nearly reached the limitation of the microscope and present methods in the elucidation of the finer structure of the nervous system, and can only hope for further advancement on the purely histological side, through the invention of new methods of technique. Ultramicroscopy has not yet contributed much to the elucidation of the subject (1).

On the other hand, experimental embryology has already made substantial contributions to our knowledge of the neurone, and its promise for the future seems bright.

The facts of experimental pathology bear perhaps only indirectly on the neurone theory. The evidence is, however, decidely favorable to this theory. Waller long ago showed that the nerve degenerated when it was separated from the cell body, *i. e.*, from the nucleus, the center of synthetic metabolism. More recent investigations have shown that correlated changes also occur in the cell body, Nissl's granules undergoing chromatolysis and the nucleus shifting to the periphery, the so-called retrograde degeneration of Nissl. Likewise, changes are found in the neurofibrils as recorded by a number of observers (Sala and Cortese, 7). These changes can best be understood on the supposition that the nerve fibers are an integral part of the cells and dependent on the nucleus and perinuclear protoplasm as the formative and nutritive center. This harmonizes with the neurone theory.

In spite of Bethe's experiments on regeneration of severed nerves in dogs, in which he thought he proved that the peripheral part was regenerated by the autogenous action of Schwann's cells, the general experimental evidence is decidedly contrary to his conclusions. Most observers have described the regeneration of the nerve as an outgrowth from the central end, and this, too, would naturally be expected if the neurone theory be true. While such regeneration was supposed to hold only for peripheral nerves, it has recently been shown that regeneration may occur to a certain extent in the central nervous system (Perrero, 6). Here, too, the outgrowth occurs from the ends still attached to the cell bodies.

That the cell body or perinuclear protoplasm is the functional and nutritive center is indicated by the fact that changes occur in Nissl's granules and the neurofibrils as the result of exhaustion, starvation, the action of many poisons such as strychnine, arsenic and alcohol, also in many psychoses. The same is indicated by the observations

of Schutz (8) on the pathological changes in nerve cells, who found that fragmentation of fibrils begins in the cell bodies and proceeds peripherally, and the repair of neurofibrils also proceeds from the nucleus to the periphery.

Experimental embryology has done more to substantiate the neurone theory than any other method of investigation. In this field the work of Harrison (3) is particularly noteworthy. He removed portions of the neural wall of frog embryos and mounting them in frog lymph was able to observe the outgrowth of the axones of the living neuroblasts and to measure the rapidity and extent of growth. The enlarged end of the growing axone presented projections which were being protruded and withdrawn, giving an appearance similar to that represented by Held and others as seen in microscopical sections of embryos. Harrison interpreted this appearance as indicating that the outgrowth of the axone is really a form of amœboid motion.

Further, Burrows (2) adapting Harrison's technique to the chick embryo and mounting portions of the neural wall in hen lymph, was able to watch the outgrowth of the axone and to measure its rate and extent. Also, he proved by using neurofibril stains that this outgrowth is accompanied by a differentiation of the neurofibrils, presenting appearances similar to those represented by Held in his diagrams of sections of the developing nervous system of the chick.

These observations prove beyond doubt that the axones of neuroblasts of frog and chick embryos when placed in suitable media are outgrowths from the cell body as the formative center, and as this outgrowth is accompanied by appearances already observed in sections and by the normal process of neurofibrillar differentiation, it can not be considered an abnormal phenomenon due to an unusual environment, but represents essentially what occurs in the bodies of the embryos. They disprove Held's statements that amœboid movement of neuroblasts does not occur and that axones can only grow out under the influence of the plasmodesms.

In summation, we may say that the weight of evidence from histology, physiology and pathology at the present time is very strongly in favor of the neurone theory, and that the evidence from experimental embryology seems conclusive that the nerve cell is a genetic unit with its formative center in the nuclear and perinuclear regions, and this we consider the essential criterion of the neurone theory.

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THE PHYSIOLOGY OF NERVE

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A valuable survey of the neuro-fibrillar theory,[1] and of observations antagonistic to the discontinuous neurone theory, has been made by Frankfurter (2) who is a partisan of the former view. "The fibrillæ are not interrupted from the spot where they take up the sensory stimulus, to their point of motor discharge"; indeed they appear to grow directly into the muscle fibers. Frankfurter admits no 'synaptic membrane' and describes the course of a fibrilla from axis-cylinder to cell-body and on to axis-cylinder, as follows:—axis-cylinder, inner network close around the nucleus of the cell-body, outer perinuclear network (surrounding the inner), dendrite, interneuronal or 'diffuse' (Apáthy) network, terminal arborization, axis-cylinder (of the next neurone). "It follows directly that all those fantastic notions, put forward specially by French authors, but also for instance by Cajal, of sleep, association, etc., as being due to active or passive movements of the dendrites—are utterly erroneous." Nor are any new connections made or paths formed in the individual's life, they merely are made 'gangbar'—'ein physiologisch leichter verständlicher Vorgang der Bahnung.' There are no 'specifically' different nerve activities, either in fibril or ganglion; nor any brain localization in the old sense. F. admits that the complete fibrillar

[1] See preceding article.

continuity and the *nervous* nature of the extracellular (diffuse) network, are still matters of dispute. v. Lenhossék (10) contends that the neuro-fibrils are not the specifically conducting elements of the nervous system; their significance is histo-genetic.

After the work on the periodic quality of the nervous impulse, stimulated mainly by H. Piper and by the advent in physiology of the string galvanometer, the most important advance in nerve physiology is, doubtless, Nernst's theory of electrical stimulation. This supposes the electrical excitation to produce its effect by concentrating the ions by which it is carried, at membranes (impermeable to the ions) which are contained within the excitable cell. Nernst's formula has in general held its own; but it fails to account for the inefficacy of slowly increasing, and of high-frequency, currents to excite nerve and other tissue. Hill (5) now develops Nernst's thought, and deduces a more adequate formula which Lucas, in an extremely well-written paper (11), shows to fit several observed facts which Nernst's own equation does not fit. This is an extension rather than a refutation, of Nernst's thought. "The characteristic time-factor in excitation is determined . . . by the rate of diffusion of the ions concerned. The factors which condition this rate are the diffusion constant of the ion concerned, and the distance between the membranes." v. Zeynek and v. Bernd (17) consider the bearing on Nernst's theory of the fact that electric oscillations of the high frequency used in wireless telegraphy do not stimulate tissues: and in general they confirm the theory. Hoorweg (7) agrees with Hill and Lucas in the inadequacy of Nernst's original 'square-root law.' Weiss (16) offers a (certainly too) simple formula which would if adopted displace that of Nernst.

Dittler (1), working with Einthoven's galvanometer, finds it probable that "the voluntary tonic muscle contraction should be considered as a muscle tetanus [cf. H. Piper], in which the action current of the muscle closely reproduces the discontinuous stimuli coming to it from the nerve. It is still an open question whether the activity of the central organ is itself discontinuous, and whether its periods coincide with those of the nerve and muscle." Höber (6) has tried to discover, by using the ultra-violet microscope, visible changes in the colloidal substance of axones as these were traversed by nervous impulses. The experiment has been so far unsuccessful. Oinuma (13) reports that for the single nerve fibril there is no submaximal mechanical stimulus, *i. e.*, if a mechanical interference stimulates the single fiber at all, it stimulates maximally.

The 'Treppe' and allied phenomena have attracted considerable experimental attention. Gotch (4) studies in the excised sciatic nerve of the frog a phenomenon previously described by Lucas in connection with the electrical response of the sartorius muscle;—this is "a modification in the time of commencement of the second response when evoked by a stimulus occurring at certain intervals after its predecessor; this time . . . may be greatly delayed and the second response appears to commence after this prolonged delay at the same moment even when the actual moment of stimulation is shifted through a considerable range of time." Thus in the sartorius muscle the true refractory period is succeeded by one in which the tissue can receive and hold a stimulation but cannot develop or propagate it. Gotch finds this same phenomenon in excised sciatic nerve. "The range of time within which the second stimulus can evoke a response with this extra delay is about 5/1000 sec. for temperatures from 3^0 to 8^0; at 12^0 C. the range is much shortened." It is not true of nerve at the higher temperatures (as it is of m. sartorius) that the second response commences at the same delayed moment even though the second stimulus is varied as regards its moment of application. "There is, in nerves at 12^0 C., distinct evidence of true nerve fatigue, showing itself only as retarded propagation." It is "present in all parts of the nerve which have been previously traversed by a sufficient succession of nerve impulses." In muscle this second response, although delayed, is generally greater than the first (Treppe), a phenomenon ordinarily attributed to facilitation (Bahnung): but owing to the conjoined delay some authors favor the phrase 'apparent facilitation' (scheinbare Bahnung). Ishikawa (8) discusses this matter. Lucas (12) finds that the delay of the second response is "due entirely to a modification of the tissue by the previous propagated disturbance," and not to any "direct local action of the previous stimulating current." Tait (14) discusses the relations between 'absolute' and 'relative' refractory phases on the one hand and the electrical 'wave' accompanying activity on the other. Tiedemann (15) discusses absolute refractory phase in a somewhat comparable manner.

A paper by Göthlin (3) must find mention even in the brief space remaining. Göthlin argues, on experimental grounds, that reinforcement of the nervous impulse takes place along the path of conduction itself (axone); that the insulation of the nerve, the myeline, is not a perfect insulator; that the electrical resistance of the axis-cylinder is very great, "and can in a moderately large (4.34μ) nerve

fiber of the corpus callosum [cow] amount to no less than 20,000,000 ohms per centimeter"; and, lastly, that the relatively low speed of nerve conduction affords no ground for denying the electrical nature of the nervous impulse.

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THE GALVANIC PHENOMENA

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There has been a considerable addition, during 1909 and 1910, to the literature concerned with electric currents obtainable from animal tissues under various conditions. The action-currents of muscles, and the so-called skin-currents have been given much attention, and the phenomena connected with the electrocardiogram have been investigated by a large number of physiologists and medical men, chiefly from the diagnostic standpoint. It is quite unnecessary and impossible to review this literature here, although it is of prime importance in connection with the study of the so-called psycho-galvanic phenomena. Barker (1) has given a most excellent historical and technical account of the electrocardiogram work, with abundant references, and this is undoubtedly the best introduction to the subject which has yet appeared. The increasing interest in electrophysiology is well illustrated by the space allotted to the topic in the new editions of two standard text books. The chapter in Stewart (16) is very full, and the chapter in Howell (5) is nearly as adequate.

The technique of the string-galvanometer, the instrument now accepted as standard for electro-physiological research, has been given due attention. Einthoven (3) has published a full account of the latest model of the instrument, and Westerlundt (19) has made a careful study of the rise in temperature and consequent decrease in sensitivity of the instrument under working conditions, attributing the lowering of the sensitivity to increased tension on the string.

Grau (4) gives new data on the influence on the cardiogram form exercised by the position of the heart, and by inspiration and expiration, and Straub (17) details the finding of a change in the galvanometric deflection produced by the intravenous injection of agents (strophantin and adrenalin) producing vaso-constriction, when the current was led off from asymmetrical locations, the effect being independent of the heart-action; but neither these nor any others of the large number of experimenters on the electrocardiogram make any attempt to bring the phenomena into relation with the so-called psycho-galvanic reaction.

Piper (10) has repeated in a more careful way his former work on the velocity of the nervous impulse, recording by the string galvanometer both the muscular action-current and the stimulus to the nerve. The rate is found to be 123 m. per sec., and to be independent

of the intensity of the excitation. Snyder (14), applying Piper's method to the measurement of the latency of the knee-jerk response, has concluded that the reflex is neural. These investigations doubtless foreshadow a wider application of electro-physiological methods to neurological problems.

The research which is of most importance for the psycho-galvanic work, is probably that of Orbeli (7), on skin-currents. Orbeli discarded the old method—the application of electrodes directly to the outer and inner surfaces of the skin—using instead salt solutions for both sides (with non-polarizing electrodes leading from the solution), or salt solution applied to outer skin (frog's leg in jar) and non-polarizing electrode bound to a surface from which the skin was removed. The use of water, or NaCl solution not over .003–.005 per cent., on the outer surface gave usually a weak current of rest directed *outwardly;* sometimes the current was directed *inwardly*, but was always weak. Stimulation of the nerve supplying the area gave in every case a response current directed *inwardly*. When NaCl or KCl solution stronger than .003–.005 per cent. was used on the outer surface, the current of rest was always directed *inwardly*, and was relatively strong. Stimulation of the nerve gave always an *outwardly* directed response current. It made no difference what solution was used on the inner skin surface. These results would seem to the reviewer to suggest strongly that the chemical action of electrolyte and skin secretions plays a large part in the production of skin-currents and in other electrical phenomena where current is led off from the skin.

In the specific field of the psycho-galvanic reflex, no great progress has been made, and the summary of Piéron (9) seems to hold true: "il n'y a là q'une méthode d'appoint particulièrement délicate dans la pratique, extrêmement dangereuse dans l'interprétation, et difficilement utilisable la plupart du temps en médicine mentale."

Veraguth (18) is strongly convinced that the current-variations are due to true action-currents from the sweat-glands, the action of belladonna in decreasing the effect seeming to speak strongly for this conclusion. Müller (6), who used battery and fluid electrodes, thinks that the effect is due to polarization, pointing out that the characteristic rest- and response-deviations are obtainable only from the palms and soles, although sweat-glands are found in other regions. The rest-current, in Müller's experiments, showed a decided drop-curve, the decrease in the deflection (d'Arsonval) being as much as 40 per cent. in six seconds. This curve could be repeated indefinitely by simply removing the hands from the solutions and replacing

them. Müller also found that the amount of skin surface immersed made a great difference in the deflection, but that the use of needle electrodes concurrently with the fluid made practically no difference, although the needle electrodes alone conducted current fairly well.

Sidis and Kalmus (12), working with the d'Arsonval galvanometer, and battery, in series with copper electrodes in concentrated NaCl solution, found that "pure ideational experiences" gave no change in the deflection, while "sudden violent emotions, and especially intense stimulation of a painful or of a disagreeable nature," produced an increase in the deflection. They found also that "muscular activity of those parts of the body actually forming the circuit bring about galvanometric deflections, while activity of the more remote parts of the body are ineffective." Curiously enough, however, "violent stirring of the liquid with the hands did not change the reading of the galvanometer."

Starch (15), working with battery and mercury-calomel-saline electrodes, found that mental processes produced a slight increase in the deflection and that emotional processes and muscular activity produced larger deflections. Apparently this is satisfactory evidence that a motor discharge is a necessary antecedent to the galvanic phenomena.

Sidis and Kalmus worked also without battery, using electrodes of unspecified type applied to the hands or hand and mouth, and found decided 'deflections.' Apparently this work was done with the capillary electrometer, as they say: "We repeated Waller's experiment, but could not confirm his results." It seems that they used much stronger solutions than did Müller, which probably accounts for the difference in results. They say: "Different experiments performed with different concentrations of electrode solutions gave different deflections, in fact, the direction and magnitude was varied at will in this way." "Different parts of the skin gave different steady deflections. However, superimposed upon this original steady deflection is a deflection due to the various stimulations given." In strong contrast with these results are the results stated by Sidis (11) in connection with experiments with metal electrodes: "We inserted platinum needle-electrodes in different parts of the body, in the same limb, or in different limbs, or but a few inches apart, and found no deflections whatever. . . . There is no current of rest." "The place of insertion of the electrodes makes no difference whatever in the galvanometric deflections." That the deflections obtained by Sidis and Kalmus depended rather directly on the action

of electrolytes on the skin secretions, is evidenced by the fact that, according to their statements, reversing the positions of the hands in the electrode solutions did not in most cases reverse the galvanometric deflections; although this may not be significant, if the experimenters were actually using the capillary electrometer and not the d'Arsonval galvanometer.

The experimenters attempted to limit the action of the skin by coating with shellac and paraffin the whole of the hand with the exception of the nails, which were left exposed. Under these conditions the characteristic deflections were obtained. Peterson and Scripture (8), however, are of the opinion that the insulation was imperfect, since they found that coating the skin with paraffin or shellac cut out the current completely. It must be noted that Müller found that coating the whole hand with a layer of lanolin and vaseline 2 mm. thick did not completely cut out the current.

Sidis and Nelson (13) worked with the d'Arsonval galvanometer without battery, with platinum electrodes inserted in the tissues of rabbits and frogs. According to their account and curves, they obtained in most cases initial deflections which in a short time returned to zero, and were renewed by muscular activity. I have pointed out in another place (2) the probability that this is a polarization effect, or at least an effect produced by action of the bodily fluids on the platinum.

The psycho-galvanic phenomenon has not yet been given a satisfactory interpretation, but several significant points have been brought out. The importance of the nature and concentration of the electrolytes, the effects of the motor discharge to the region from which the current is taken, or regions through which the current flows, and the influence of drugs acting on the glandular and vascular systems, have been brought out in a way which indicates the line of future research.

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NOTES AND NEWS

The Fourth International Congress of Philosophy was held at Bologna April 6–11. One of the eight sections was devoted to psychology. On the preliminary circular there were announced for this section sixteen titles covering a wide range of psychological topics.

Nature announces that plans are well under way for the erection at Cambridge of a building for the projected psychological laboratory.

Professor G. M. Whipple, of Cornell University, will give two courses at the coming summer session of Columbia University. The subjects of the courses will be, respectively, Mental Development, and Psychological and Educational Measurements. Professor Kirkpatrick, of the Fitchburg Normal School, will replace Professor Whipple at the Cornell summer school and offer courses in educational psychology.

The third annual meeting of the Minnesota Psychological Conference was held at the University of Minnesota March 31, 1911. The program was devoted chiefly to the educational applications of psychology.

The present number of the BULLETIN, dealing especially with nerve physiology, has been prepared under the editorial care of Roswell P. Angier.

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THE

PSYCHOLOGICAL BULLETIN

GENERAL REVIEWS AND SUMMARIES

TASTE AND SMELL

BY PROFESSOR E. A. McC. GAMBLE

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The last year has brought no epoch-making contributions either to the psychology or to the physiology of smell and taste. Larguier des Bancels (6) makes an excellent transcript of the sum of knowledge in regard to smell, a transcript which is the counterpart of his recent paper on taste (6). Sternberg (7) writes rather discursively on the perception of differences in taste and smell. The differences which he has chiefly in mind are the gross differences between the four taste qualities and between such smells as valerian and pyridin. He has experimented upon cats, upon a human anencephalus, upon some suckling infants, upon twelve children between eight and fourteen years of age, four blind, four deaf-mutes, and four normal, and upon certain sick children and adults. The experiments were probably performed by clinical rather than by academic methods for the writer gives no details of procedure aside from mentioning the materials and instruments used. His vapor gustometer and olfactometer are of interest. Cuts are included in this paper. The writer holds that the mechanism for sharply different reactions to different tastes is established in the human fœtus before birth. Indeed, the anencephalus reacted very differently to sweet and bitter. The lack of sight or hearing seems to better the differential sensitivity for taste and smell. Cats are highly sensitive to taste and smell differences. Sensitiveness to taste differences is heightened in gastric disorders.

Becher (1), who, in connection with an investigation of the physiological effects of alcohol, has recently studied fifty persons over

ninety years of age, reports two cases in which aged women showed an abnormal desire for sweet as compared with unsweetened foods. In one case, even bouillon was taken only when sweetened with cane sugar. Becher believes that we have here a hyperæsthesia for salt, sour and bitter tastes and a desire for sugar as a corrective. This senile hypergeusia he believes to be rare but to be akin to the hypergeusia of hysterics and neurasthenics, to senile pruritus, and perhaps also to pathological *libido senilis*. Urbantschitsch (8) has studied in thirty cases the effect of unilateral inflammation of the middle ear upon keenness of smell on the affected side of the head, taking the keenness of the other nostril as a standard. He used five kinds of odors (not named). In some cases, sensibility was impaired for all the odors on the side of the diseased ear, but in other cases it was heightened, whereas in still others it was heightened for some odors but impaired for others. The writer concludes that suppuration in the middle ear is sufficient to produce disturbances of smell without otogenous abscess in the temporal lobe as an intermediary.

Hopf and Edzard (5) have studied the distribution of the lingual papillæ in different human races and in apes. Their subjects comprised seven negroes, five Melanesians, one Javan, and three Japanese, six gibbons, one orang, and one chimpanzee. No striking differences were observed in either the fungiform or the filiform papillæ or in the number of circumvallate papillæ. Certain slight racial differences did appear in the arrangement of circumvallate papillæ. In this regard, the Melanesians and gibbons departed most widely from the Caucasian, human type. No very definite findings in regard to foliate papillæ were made.

Botezat (2) has made an extended study of the taste organs of birds,—sparrows, linnets and water-rails. The taste-buds of birds resemble in structure those of man, but are often different in shape, are not found on definite papillæ, and occur mainly in the throat, although they appear also in the forward part of the mouth cavity except on the tip of the tongue and the forward part of the hard palate. They are of two sorts, independent buds, which occur in groups both in ordinary and in glandular epithelium, and gland buds, which are found about the mouths of glands, one, two, or three to a gland. The functional importance of both kinds of buds is probably slight, but whereas the independent buds correspond to the taste buds of the lower vertebrates and to the ordinary taste buds of mammals, the gland buds are the forerunners of the more sensitive buds about the circumvallate papillæ of the higher mammals.

In ascending the scale of vertebrates, the area over which the taste buds are distributed diminishes, and in the mouth cavity, with the disappearance of the buds, comes a furrowing of the epidermis and a formation of corium papillæ which, in the higher mammals, are in part tactile in function. Gawrilenko (4) has studied the development of the smell organ in the salmon trout. Of interest are the descriptions and plates which exhibit the doubling of the olfactory pouch on each side, a doubling which corresponds to the division of olfactory nerve fibers. The medial half of the pouch is the original of Jacobson's organ; the lateral half is the forerunner of the olfactory membrane in mammals.

Von Frey (3) proves abundantly by experimental data that the 'alkaline taste' is really an alkaline smell. He shows that it is due to the liberation by the alkaline substance of the volatile base, methylated ammonia, which is formed from the decomposition products of the buccal epithelium and is found in ordinary but not in freshly secreted saliva.

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CUTANEOUS, KINÆSTHETIC AND MISCELLANEOUS SENSES

BY PROFESSOR ROSWELL P. ANGIER

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During the year 1910, v. Frey (7) published a short review of work on the physiology of the cutaneous sense organs, beginning

with that of E. H. Weber, and Kiesow (9) prefaces his preliminary report on reaction times to cutaneous pain by a twenty-page account, admirably written, of the development of our knowledge of pain sensations since the time of J. Müller and E. H. Weber.

Cutaneous Sensations.—Baglioni and Tamburini are concerned with the existence of a specific electrical sense. Tamburini (16), on purely introspective grounds (*e. g.*, that the contact of two electrodes with the skin gives, when the current passes, a sensation qualitatively different from that obtained with no current), affirms the existence of a special electrical sense, even although evolution has not yet developed special terminal organs. Baglioni (1), working under more rigid experimental conditions, concludes that there is no elementary electrical sense but that the faradic current is an inadequate stimulus for touch and pain and, agreeing with a previous statement of v. Frey, for temperature organs. He first found that the degrees of sensibiltiy to faradic stimulation for the regions studied correspond with those for touch proper in the same regions. On then testing the dorsal surface of the glans penis, which v. Frey had found insensitive to touch, he evoked a pain sensation at 10 volts, sensing nothing at all similar to touch. Some of the results for other portions of the body were: *tip of tongue*, touch ('formicolio'—slight prickly sensation) at .5 volt, pain at 7 volts; *inside of upper lip*, touch at 4, pain at 9 volts; *back of hand*, touch at 25 volts, and prickle ('puntura') at 30 volts. The faradic current of low intensity excites, therefore, the touch and, of higher intensity, the pain organs.

The year yields two investigations on the spatial distribution of sensitive points in the skin. Franz (6) determined touch thresholds for selected points in representative bodily areas on five women subjects, four normal and one a neurological case. Bloch's instrument was used. For the detailed results one must consult the original article, where tables for the thresholds of each subject are given and also a comparative table showing the gradings of sensitivity for the various areas. Elo and Nikula (3) plot a topography of thresholds for warmth. Each experimented on the other, the leg of one being investigated and the trunk, arm, neck and head of the other. They used the flattened lower end of a thermometer, covering a skin area of about ½ sq. cm., as the stimulating surface. The bulb, just above this flattened surface, was wound with insulated wire, connected with resistances, so that any desired temperature could be obtained and easily read off from the thermometer itself. In order to avoid the actual handling of this thermometer-stimulus, a wooden

handle projected at a right angle from it. Each application of the stimulus lasted about 1 second. The experiment room was kept at a constant temperature of 22° C. Only hairless areas were stimulated and those portions of the body usually covered by clothes were exposed only while being tested, since it was found that when the leg, for instance, became adapted to the air, the thresholds were lowered. In some cases a given threshold obtained for only a small area, in other cases for a much larger area. For points directly over a bone with little flesh covering higher thresholds obtained than for immediately adjacent fleshier points. Some idea of the spatial distribution of thresholds may be obtained from the following figures, all of which indicate degrees Centigrade: face, 31°–37°; neck, 24°–33°; breast, 30°–32°; abdomen, 34°; about anterior pelvis, 36°; back, 30°–33°; arm (upper), 28°–31.5°; forearm, 31.5°–33°; leg (inner upper), 30°–33°; outer upper, 32°–34°; inner lower, 33°–35°; outer lower, 32°–35°.

Allied to these topographical investigations are those of Franz, and Baglioni and Pilotti. Franz[1] (4) recurs to the problem of cutaneous sensations following nerve division, previously investigated by Head, Rivers and Sherren. In Franz's subject (a male nurse, an intelligent and careful observer), through a cut, the ulnar nerve had been divided, the median nicked and the medial antibrachial probably severed. The main examination for sensibility began about two months after the suturing of the divided ulnar. Head, Rivers and Sherren having found three different types of sensibility which, in general, might be separately lost or recovered,—*deep* sensibility (pressure, movement of parts, etc.), *protopathic* sensibility (cutaneous pain, extremes of heat and cold) and *epicritic* sensibility (light touch, moderate temperatures),—inferred that there are three independent systems of nerve supply and not merely an overlapping of areas of supply from the same system. In general Franz verifies these findings. He found hairs insensitive to traction, for instance, when still sensitive to light brushing, a fact in accord with the finding of a twofold nerve supply to the hairs. This is not to be explained by mere variation in threshold, since the traction stimuli were intenser than the brushing. The results from temperature stimulation also warrant the assumption of different nerve supplies for extreme (protopathic) and mean (epicritic) temperatures respectively. Although, for instance, extremes of temperature on areas not responsive to mean temperature evoke the responses 'cool' or 'warm,' the explana-

[1] Part of the material of this article appeared in the *Jour. of Comp. Neurol. and Psychol.*, 1909, 19, 107–123.

tion cannot lie in mere threshold differences, since in some areas sensitivity to extremes is lost but that to means retained. Here, therefore, as above in the case of hair sensibility, the criticism generally made of the results of Head and others (that they are all explicable by merely assuming differences in threshold) cannot be upheld. Franz is inclined, however, to assume more extensive overlapping than his predecessors. He found, for instance, in testing the area intervening between an area of total loss of sensibility and that of normal sensibility, a gradual lowering of the threshold from the former to the latter. This implies an overlapping of nerve supply.

The work of Baglioni and Pilotti (2) concerns spatial and temporal order of disappearance (or reappearance) of the various forms of cutaneous sensibility under the influence of stovaine. The complete anæsthesia spreads gradually over the lower extremities, following an areal order *strictly corresponding to the order of arrangement of the fibres of the sensory spinal roots in relation to the point of stovaine injection.* It appears, therefore, that the stovaine affects these root fibres rather than the central paths in the cord, acting as a 'Blockadeerscheinung.' Secondly, the four cutaneous senses, for any given region of the skin, do not disappear at the same time; pain goes first, cold next, then warm, and finally pressure. Physiologically this is more difficult of explanation, if one assumes qualitative identity in the nervous impulses of the various afferent fibres. The authors are therefore inclined to assume that this differential effect of the anæsthetic on the various fibres indicates that the nerve impulses are not qualitatively identical,—an argument in support of Hering's theory of nervous activity. The order of return of sensibility in both the foregoing instances is the reverse of that of disappearance. Further, for a given quantity of stovaine, there is a differential spread of the four types of sensation, complete anæsthesia existing for a certain region but, beyond, only analgesia. Perhaps as interesting as any of the authors' facts is a striking demonstration of a paradoxical sensation of warmth, — a phenomenon not observed in the normal subject. Between the time of disappearance of sensitivity to cold and that to warm stimuli (or between their respective reappearances) the application of a vessel (30 mm. diameter) containing ice evoked the sensation warm.

Somewhat allied to this investigation is that of Franz and Ruediger (5) on sensory changes following the application of ethyl chloride. An area on the forearm, 5 cm. square, was marked off. The central part was sprayed with ethyl chloride until freezing set in, frost appear-

ing on the surface. This inner area was then tested during different stages of the chloride effect, and compared with the surrounding normal area. Ethyl chloride was found to be an analgesic and an anæsthetic. The latter effect was, however, shorter in duration than the former. Sensations of light touch, from slightly brushing the hairs, recurred very soon, while those of pressure and pain, from pulling the hairs, were absent for a longer period. These facts, like the similar phenomena recorded above on cutaneous sensitivity after nerve division, indicate two distinct sensory end organs for the hairs, contradicting the assumption that pulling sensations are merely the exaggeration of the touch-like sensation obtained by light brushing. Forthermore, pain from hair traction returns sooner than pain from pressure, a fact again indicating two separate end organs for these two types of stimuli.

Voigt (18) has studied in a preliminary way temperature judgments under the influence of adaptation. Three vessels, each containing about 3¾ litres of water were used. The left hand was immersed in vessel no. 1 and the right in vessel no. 2 as far as the knuckles. The water in vessel no. 2 differed in temperature from that in vessel no. 1 by 5° or 10° C. Immersion continued for from nine to fifteen minutes, special control experiments having shown that the degree of adaptation did not alter, whether the hands were kept in the water two minutes or seventy minutes. After adaptation both hands were plunged into vessels 2 and 3 respectively, the left in vessel no. 2 and the right in vessel no. 3. The subject was then asked whether the temperature in vessel no. 3 (for the right hand) was greater or less than, or equal to, that in vessel no. 2 (for the left hand). If adaptation had been complete for both hands, *i. e.*, so that the temperatures for vessels 1 and 2 felt alike at the end of the adaptation time, then a temperature difference between vessels 3 and 2 equal to that between vessels 2 and 1 should be felt as equal for both hands. This, however, never happened,—the ratio of the difference between temperatures in vessels 2 and 3, *when felt as equal*, to the difference between those of vessels 1 and 2, ranged from .1 to .25, instead of 1.0, which it would have been had the adaptation been complete. It appears thus that the degree of adaptability of the temperature organs in the hand is very small, and never as high as a fourth of complete or ideal adaptation.

Moore (11) reports on the adaptive influence of temperature and the electric current on skin sensibility. For temperature a hot water bag was placed on the volar surface of the forearm. The temperature

of a thermometer placed between the bag and the arm was taken as the arithmetical mean between the two. The sensitivity was determined by a series of v. Frey's hair æsthesiometers. The threshold for touch was lowered, under adaptation, up to a (calculated) skin temperature of 35°–38° C. For higher temperatures it rose again. As for electricity Moore found that the *direct* current decreases sensibility at the anode and increases it at the cathode, except for very strong or very weak stimuli, when the effect is doubtless concealed by the creeping of the anodic effect towards the cathode or vice versa. These phenomena are analogous to Pflüger's law of irritability in muscle. The *induced* current, immediately after application, lowers skin sensibility for touch and pain and a touch spot was found under these conditions to be transformed into a pain spot. In discussing the significance of these results the author points to the close parallelism between the temperature for maximal sensibility and that for maximal dissociation in bodily tissues, as well as to that for the maximal irritability in muscle. He concludes: "The maximum sensibility for touch and the maximum for muscular irritability are both found at the temperature of the greatest dissociation" (p. 374). The diverse functions of muscle, nerve, and sensory end organs appear, therefore, to be dependent upon one and the same chemical phenomenon.

Weber (19), with a different end in view, likewise tests the influence of temperature on touch sensibility. The forearm was warmed in heated air. For about six minutes after warming, as long, in fact, as the arm *felt* warm, sensibility diminished, but then increased, the increase persisting for 15 minutes or more. The preliminary decrease in sensibility was probably due to the effect of the temperature on the nerve endings themselves. When this passed off the dilative effect of the heat on the capillary blood vessels of the skin (arterial hyperæmia) became manifest in the lowering of the threshold for touch. The conditions of the experiments involved fixed attention by the subject to the area touched and, in order to accentuate and direct this, a preliminary stimulus markedly above the threshold was applied. Plethysmographic tests were then made to find whether the concentration of attention on a definitely localized skin area itself resulted in an increase of peripheral blood supply to that area. Among other tests, both arms of a normal subject were inserted in plethysmographs, the record being taken, however, for one arm only. In order to fixate attention, slight vibrations of the table were made, which were felt by both arms. Whenever the arm for which records

were taken was attended to there followed an increase in the blood supply and a decrease when the other arm was attended to. Such results certainly serve as a warning that the direction of attention is a powerful influence in determining experimental results.

Before closing the account of cutaneous sensations reference may be made to a preliminary article by Kiesow (9), already mentioned in another connection, which concerns reaction time, however, more than sensation. Two articles by Sternberg, on tickling and itching sensations may be passed over lightly. In one (13) he makes the tickling in the mouth by solid foods an essential part of our craving for such foods, creating an irresistible call for the energetic filling of the stomach with solids. He sees in this the reason why men could not go long on liquid food, and why babies suddenly begin to crave it. In the other article (14) he sees no reason for distinguishing between tickling and itching sensations, the difference being merely external; the former is the sensation when we are intentionally stimulated, the latter when a stimulus is inner or unknown. One is evoked actively, the other passively. He denies that subjective attitude plays an essential part in tickling, as well as the supposed fact on which such assumption is based (*i. e.*, that one cannot tickle himself), for if one runs the tip of the tongue or finger lightly from back to front on the roof of the mouth, one gets intense tickling sensations.

Organic Sensations.—Ritter (12) shows that a dog, narcotized with morphine, shows marked pain reactions when the viscera are stimulated. The contrary results of earlier investigators are doubtless due to the injury of the finer sensory fibres resulting from their extensive laparotomy or from long exposure of the viscera to air, which invariably decreases sensibility. Ritter made only small incisions and tested immediately but a small portion of exposed tissue. The vascular tissue is the most sensitive, except the mesentery. He is inclined to think this sensibility to pain also characteristic of human beings, partly as the result of experiments on six such subjects. Torata (17) found no reactions in an animal under strychnine to touch or cutting, from the heart or other inner organs; since strychnine causes tactual hyperæsthesia in the external skin, he concludes that the sensitivity of the internal organs is essentially different from that of the external. Without strychnine the heart, the stomach and the beginning of the intestine, subjected to cutting and chemical stimuli, caused responses. Miller (10) finds that a warm mustard solution, introduced directly into the stomach of an anæsthetized cat, causes vomiting and that the *vagi* are the afferent nerves concerned. Haudek

and Stigler (8), by means of the Roentgen ray, found that the stomach is emptied more rapidly after a meal eaten with relish ('Esslust') than when it is not. He makes no causal inferences. Sternberg (15) disagrees with Pawlow's assumption that appetite is the correlative of the secretion of juices in the mouth and stomach since (1) other things may occasion such secretions, (2) appetite is accompanied by other physiological phenomena and (3) these secretions, otherwise stimulated, do not occasion appetite.

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SYNÆSTHESIA

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In the course of presenting some of the abnormal and unusual features of adolescence, Lemaitre (3) devotes two chapters (pp. 15–74) to his findings in respect to synæsthesias. No unusual varieties are reported, though, naturally, the cases cited are not without individual interest. Photisms of one variety or another and more or less extensive in character are found, it is claimed, in about one-fourth of adolescents. The period seems, however, to be critical for these states, about one-half losing by the age of fifteen of sixteen the previously possessed photism or other form.

From an examination of thirty-seven cases of colored hearing a table is compiled (p. 23) showing the distribution of colors in respect to the vowels. The tabulation—useful only as a basis of comparison with similar collective accounts, and then only when it is remembered that the French sounds of the vowels are in question—shows "that *a* is usually red or white, that *e* is yellow, that *i* is white, that *o* is black, that *u* is blue with a leaning towards the green or the yellow."

The second of the two chapters devoted to synæsthesias recalls, and in some particulars completes, three cases described by the author in 1901, in a volume entitled *L'audition colorée et les Phenomènes connexes*.

Mercante (4) is also concerned with the distribution of synæsthesias among children and adolescents. Examining upwards of 900 pupils of both sexes, he finds that about 80 per cent. (the girls somewhat in excess of the boys) present some form of chromæsthesia. Such experiences the author regards as quite normal for the ages between 8 and 18. A marked concordance in the coloration of the vowels, and of certain words, leads the author to believe that the chromæsthesias develop through the presence of a common affective ground, the given vowel and the associated color being both natural and usual excitants of the same emotional tone.

Ferrari (2) reports a case of 'gustatory audition' communicated to him by a young doctor of science who was himself the subject. The hearing of proper names and ordinary nouns evokes tastes and

odors. Examples are given, but no data are accessible for interpreting the origin or development of the associations.

So far as the present writer is aware, this is the third case of this variety of synæsthesia to be reported. In 1907, only two months after my own report of a similar case, Ferrari published in the *Rivista* an extended account of the experiences of a young woman, Nerina U. Taste, smell, and food equivalents were given for two hundred proper names, which alone seem to have evoked the experience. In general Ferrari's case agrees with my own. It differs from it, however, (1) in that odor equivalents are present (my own subject was anosmic), and (2) in not revealing the wide range of response to sounds of all sorts. It is interesting also to note that Ferrari's subject, while not presenting a serious defect, showed idiosyncrasies of taste and smell, preferring bitter to sweet and being fond of odors (like that of petroleum) which are usually disliked. This case seems to be so little known that its mention here may, perhaps, not be out of place.

Miss Downey (1) relates a case of colored gustation found in a young man with a somewhat defective sense of taste. The taste-colors, which were clear and precise, "were definitely localized in the mouth, where of hallucinatory vividness, and had a uniform color-tone and persistence under constant conditions." The localization of the color in the mouth, while here not unnatural, is not unique, the present writer having recently found a similar localization in a case of colored hearing.

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AFFECTIVE PHENOMENA—EXPERIMENTAL

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The most extensive publication of the year was that of Weber (7). He studied the arm volume by means of the Lehmann plethysmograph. The volume of the ear was recorded by a capsule which fitted over

the ear and was connected to the registering apparatus. Changes in the abdominal vessels were tested with a small rubber bag inserted into the rectum and weakly inflated. Weber states that mental work, strain and shock gave a fall of volume of the arm and ear, and an increase in volume of the abdomen. Changes of level in the ear were more abrupt than those in the arm. Agreeable feelings and agreeably-toned emotions are said to be accompanied by rise of volume of the arm and ear and fall of volume of the abdomen; disagreeable states show opposite effects. A part of the stimuli for these results consisted of agreeable or disagreeable tastes, noises and problems; but most of the supposed effects were due to suggested stimuli during hypnotism. A stimulus will cause some reaction even as an unconscious process, but Weber believes that the suggestion in consciousness is the primary factor.

Electrical stimulation of different parts of the motor cortex of animals gave a rise of blood pressure, fall of volume of the abdomen and the ear, due to active constriction, and rise of volume of the extremities, due partly to active dilation. The use of this reaction is to send blood from the abdomen to the muscles concerned. The part of the motor cortex effective in different animals varies with the muscle-group most concerned in the mode of life of the animals. Similarly, Weber asserts, voluntary movement, or an idea of movement without actual movement, leads to rise of blood pressure, fall of volume of the ear and the abdomen, and rise of volume of the extremities, especially of the member most concerned in the intended movement. Many of these results were obtained during hypnotism.

Weber attempted to test these statements as to blood distribution by means of the Mosso balance. But he improved the method by making the board longer, and balancing the subject with the abdominal organs first on one side of the axis, then on the other. The movement of the writing-point was opposite in the two cases. Hypnotic suggestion was used again. The statement is that with ideas of movement, agreeable conditions, and sleep, the centre of gravity tends to leave the abdomen (the side on which it is placed becoming lighter); with other processes one gets the opposite result. The author obtained three curves, showing the influence of disagreeable stimuli upon the brain circulation. They are much obscured by movement, but he draws the conclusion that there is a decrease of volume and size of pulse.

The innervation of the cerebral vessels was studied by recording the arterial pressure and the brain volume in animals. After section

of the cord, or destruction of the general vasomotor centre, Weber stimulated the cortex and the possible nerve supply to the brain, and the conclusion is reached that the brain vessels are independent of the centre in the medulla, but are controlled by a separate centre centralwards from the medulla. The two hemispheres respond alike.

The author does not accept entirely the Lehmann-Berger theory of feeling. Particularly, he finds that with disagreeable stimuli, the cerebral vessels constrict at once in order to prevent too violent dissimilation; the peripheral vessels constrict in order to lower the sensitivity of the nerve terminals to the disagreeable stimulus.

It is difficult to evaluate the work of Weber. We certainly cannot accept results under hypnotism, especially when we know practically nothing of the relation of hypnotism to the circulation. His curves are often too short and many of them seem to be entirely ruined by movement. The results on cerebral innervation are suggestive. But as to the feelings, there are no adequate introspections, too few results, and the work is in no way comparable with several others that have appeared.

A controversy between Weber (8) and Berger (2) leads to the statement that disagreeable sensations often cause vasoconstriction in the brain. Berger (1) found that mental work caused some rise of temperature in the brain; the shock of a sudden noise caused a much delayed and smaller rise. The whole method is doubtful and the discussion very speculative.

After an excellent review of the literature of the last ten years, Kelchner (4) considers the following points reasonably well established: (1) The organic accompaniments of the feelings in the circulation and in the breathing are not on the same plane; in the latter, voluntary as well as reflex and automatic modifications must be considered. (2) The vasomotor changes are not primary in relation to the other organic changes. (3) The reactions are in part central, in part subcortical. Subliminal stimuli during sleep, narcosis, and hypnosis are not without effect; and there seems to be a dependence of certain reactions upon the kind of stimulus. (4) The emotional organic reactions are determined by the total psychophysical disposition. (5) The organic reactions are, at least in part, of significance for the functioning of the central organs. (6) It has not been shown that there is a delay of the expression after the feeling. (7) No observation contradicts the fundamental ideas of the peripheral theory of feeling.

Dumas and Malloizel (3) have shown that sexual excitement and

anger cause increased salivary, renal, and gastric secretions. Fear was more ambiguous in its effects.

Wells (9) tested the affective character of colors with a large class of students. The spectrum was divided into twelve hues. These were painted upon two-inch squares, mounted upon neutral gray, and numbered. The subjects were asked to designate the mood suggested by the color. Crimson, scarlet, deep orange, orange-yellow, and yellow were dominantly exciting. The middle region of the spectrum was tranquilizing. Violet-blue, violet, and purple were especially depressing. The different replies for the middle, tranquil region were often contradictory; those for the extremes were quite uniform as to exciting or depressing character. It is noteworthy that these characters persisted whether the subject liked or disliked the color. Scarlet was always exciting, whether it was called cheerful or loud.

Taylor and Washburn (6) obtained the affective reaction of a number of students to logical fallacies. The sources of unpleasantness that may be called logical were, beginning with the most indefinite, general sense of confusion, sense of something omitted, sense of a lack of equivalence between terms, clear idea of the omitted factors.

A second edition of Lange's *Die Gemütsbewegungen* (5) has appeared.

On the whole, the year was poor in valuable experimental results.

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AFFECTIVE PHENOMENA—DESCRIPTIVE AND THEORETICAL

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A comprehensive critical review of work in the field of affective psychology during the ten years, 1900–1909, is given by Kelchner (7), who notices no less than 147 titles, all bearing on fundamental problems. This, not including the Italian literature, fairly represents the present lively interest in the subject. There is little to record of unanimity in results, and tendencies often seem more conflicting than converging. Four leading conceptions of the nature of feeling (including emotion) have been developed, with no agreement as to its criteria. Kelchner reviews the discussion of the peripheral theory *in extenso*, concluding that while the pathological evidence is ambiguous, investigations into visceral sensibility show that the theory is possible, while no observation obtained by the method of expression refutes its fundamental idea. As to the qualities of feeling, opinion appears on the whole to be against Wundt's tridimensional theory; it is suggested that the proposed new dimensions may be better characterized by relating them to moods than by comparison with simple feelings. A masterly analysis of the entire field of present discussion is also given by Külpe (8) in a paper read at the Geneva Congress in 1909. Külpe regards 'feeling' as an elementary mental state characterized by (*a*) universality of the stimuli,—anything can produce it, and (*b*) actuality,—it is always capable of being realized. These marks, he holds, are only found combined in pleasantness and unpleasantness; Wundt's proposed additions he takes as general sensations and the so-called intellectual feelings as conceptual marks of the object and general unanalyzable impressions. The admitted qualities are always identical, but may be variously classified as particular and general, active and passive, etc. The most noteworthy feature of Külpe's paper is his attempt to classify and analyze the various methods of investigating feeling. All are complex and relate as well to the production of the impression as to the subjective state of the observer. Külpe enumerates the elements of procedure in each group of methods, and finds, *e. g.*, no fewer than fourteen such particulars in the methods of registering the reactions. Experimentalists will take note of this analysis as both serving to re-establish synthetically the old and leading to new methods of research. The general nature of affective consciousness is also the

topic of the first of the articles in a volume by Ribot (16). Ribot's contention is that affective consciousness is of a totally different order from cognitive consciousness, being an expression of the organic, vegetative life. For this he offers evidence both direct and indirect, direct, *e. g.*, in certain experiences of pleasure, pain, fear, etc., which are devoid, or nearly so, of cognitive content, indirect, in the disappearance, or extreme enfeeblement, of one factor without the other. Ribot rejects Wundt's 'tension-relaxation' as belonging to the form rather than to the matter of the feeling, but includes 'excitement-repose,' which, as lying more in the region of the subconscious, affords, he thinks, a basis for admitting a 'state of indifference' intermediate between the pleasant and the unpleasant.

Another of Ribot's essays deals with the theory of pleasure, as does also that of Gardiner (4). Ribot's thesis is that pleasure is 'a higher form of normal life,' the normal state being affectively neutral. He traces its development through three stages,—euphoria, which is opposed, not to pain, but to dysphoria; pleasure determined by relation to an object; and pleasure as an exuberant activity. Nevertheless he rejects the theory which attributes pleasure to surplus vital energy and reverts to the older view of Aristotle, which makes it the accompaniment of unimpeded activity of tendencies. This is essentially the view reached by Gardiner as the result of a direct confrontation of ancient and modern theories. Pleasure, he holds, as expressing and furthering a function exercised in conformity with present tendency, is a sign of enhanced vitality, not, however, of the organism, but of the tendency and function.

The view that feeling is a unique mental element incapable of presentation and showing no tendency to ideal revival gives rise to the difficulty, or even denial, of 'affective memory' and, indeed, of understanding how feeling, as such, can be apprehended at all. The fact of affective memory, *i. e.*, of the revival of past affective experience with recognition of its pastness, is vigorously contended for by Ribot (16). His proofs are partly psychological,—comparison of past and present affective states (*e. g.*, the case of St. Preux), and the emergence of a feeling vaguely referred to the past and gradually becoming definite by reinstatement of the appropriate intellectual images; partly physiological,—it is incredible that the affective processes should always fail of revival when their intellectual concomitants are revived; partly pathological,—*e. g.*, nostalgia is often cured by a visit home, showing that the affective memory and the actual impression are distinct; partly indirect,—*e. g.*, the develop-

ment of 'general' emotions by repetition. It is, of course, not contended that the revived affection is visually imaged. Wodehouse (19), discussing the general question from the point of view of an adherent of Ward's doctrine, denies that it is imaged at all. She holds that any past experience can be apprehended in thought, as the subject of judgments, without repetition, either actual or make-believe, and that past feeling is, in this respect, in the same position as the presented elements of consciousness. The inference as regards present feeling is that we only know it "when our present act is to think of our present feeling." The whole difficulty appears to the reviewer to be largely fictitious. No one denies, even while declaring that feeling cannot be presented, that we are conscious of it; no one denies that feelings can be in some sense revived; and if it is maintained that such revived feelings are actual feelings, might not the same thing be said, with the qualifying explanations, of a visual or other sensory image? The truth seems to be that there is no such opposition of 'feeling' and 'presentation' as is presupposed, but only difference in the functional relationships of distinguishable conscious contents. The imperfection of our knowledge of our own feelings is well set forth by Ribot (16) in an essay on a form of affective illusion. We falsely judge their quality, genuineness, persistence, etc. This is explained as due partly to the impossibility, or extreme difficulty, of direct comparison, partly to suggestion, including auto-suggestion, but principally to the impossibility of knowing all the factors which determine the feeling, these being not only conscious, but subconscious and unconscious.

The doctrine that feelings are at once revived, individually experienced and yet 'represented' is an essential part of Prandtl's theory of empirical 'Einfühlung' (14). Prandtl criticises and rejects the imitation theory of Lipps in favor of a purely associationist theory, according to which, however, certain factors in the association, though present, may be unconscious. The usual order of the process he represents as follows: the visual perception of another's demeanor revives the idea of one's own similar appearance; this redintegrates the associated kinæsthetic and other organic sensations, and these reproduce the concomitant feeling, which is then, by a 'splitting' of the contents present, interjected into the other as the interpretation of the original perception. Considerable ingenuity is shown in explaining and defending this theory; but it may be questioned whether, in discarding all intermediation of imitation, any such explanation can be complete. Besides empirical 'Einfühlung,' in

which a feeling of mine is introjected into and terminates in another 'similar' object, the author treats of 'Stimmungseinfühlung,' in which my feeling does not relate itself to anything definite, but colors the situation while still felt as actually experienced by me.

The dulling of feeling is discussed by Martin (10), who finds no reason in the nature of adaptation why an agreeable emotion of moderate intensity should be dulled by repetition and regards it as improbable that painful or too violent emotions are dulled in this way alone. The conditions affecting dulling are, according to him, five: imaginative idealization of the object, surprise or expectation, fatigue, the need of enriching mental or emotional experience, and voluntary or involuntary attention.

To the psychology of the 'intellectual' feelings Piéron (12) contributes a brief paper of description and comment on a prolonged and repeated effort, finally successful, to recall forgotten names. The feelings in question are those of nearly attaining, of being balked, etc. The conclusion is that in a general way these 'feelings' appear to be sensations resulting from the cerebral dynamism facilitated (feeling of comprehension, recognition) or inhibited (feeling of novelty, obscurity, etc.), but that it is very difficult to verify this hypothesis.

The subject of the emotion is treated in a quasi-literary way by Rivari (17), who comments on the descriptions of fear, anger, etc., in Dante's *Inferno*. Three papers, among those under review, besides the previously mentioned article of Kelchner, deal with the theory of emotion. The problem of Tassy (18) is to account for the immediacy with which an emotionally exciting idea, *e. g.*, that of danger, connects itself with a specific organic reaction. He finds the solution in some original disturbance in the adaptation of the neurones concerned in ideation. Certain objects in virtue of their magnitude, unexpectedness, etc., set up reflexes in these elements which tend not to admit them. The object may be clearly represented later, the disturbance is directly represented and the organic correspondences, 'organic fear,' which the disturbance sets up are also represented. These three representations are associated. When, then, the object is again presented, the total association, if the memory of the disturbance is sufficiently intense, is revived. The idea of danger is acquired on the basis of the mental state thus mechanized. This accords with the peripheral theory, which Piéron (12) and Lapicque (9) consider as now definitely undermined. Piéron reports the recent experiments of Gemelli, who operated on two dogs and a cat, combining the methods of Sherrington, Bechterew

and Pagano, all of whose results were confirmed. But while the apæsthetized but non-decerebrated animal reacted normally to novel as well as to habitual stimuli, stimuli applied to the one that was both apæsthetized and decerebrated seemed to set up mere reflexes; moreover, when the *n. caudatus* of this animal was stimulated, the reactions showed no emotional phenomena, though normal in other respects. Piéron, admitting that he was wrong in formerly regarding the *n. caudatus* as the 'seat' of the emotion, since the cortex is here shown to be necessary, finds in these results evidence of the secondary rôle in emotion of the peripheral cœnæsthesia. Lapicque agrees with this, yet holds that the 'expression' forms an integral part of the total representation of an emotion. His theory of the process rests on a conception of discontinuity in structure and function of the elements of the organism, manifested in differences in the unit of time ('chronoaxia') required for effective response to stimulation. If the influx is of moderate intensity, the excitement, on the theory, passes to connected neurones of isochronous rate; if greater, to others that are heterochronous. It is only in the latter case that we have emotion. Emotion is thus regarded as due to a generalized irradiation, based primarily on intensity of stimulation, though this may come not from the initial stimulus, but from influences from other centres with which a given neurone or group of neurones is functionally related. A somewhat different irradiation theory is formulated by Bawden (1) and applied to the explanation of the comic. All emotional phenomena are held by him to be conditioned on an initial summation of stimuli, followed by an irradiation or discharge. The comic is connected with relatively abrupt relief from the tension accompanying the vigorous exploiting of habit-systems. The rudiment of æsthetic experience is accordingly found in primitive laughter.

Several studies besides Bawden's deal with specially named emotional states. De Fursac (3) treats of the passion of the miser, the essential elements of which he finds in exaggeration of the saving instinct and, negatively, atrophy of the affections and restricted mental activity. Hence whatever tends to weaken the former and, again, to develop the latter is unfavorable to the passion, and *vice versa*. These principles are then applied to the relevant facts. Two writers, Queyrat (15) and Ingegnieros (6) treat of curiosity. Queyrat's aim is practical. He distinguishes the forms of curiosity under such terms of appreciation as 'frivolous,' 'malign,' 'fruitful,' the interest culminating in a chapter on the education of curiosity. On the

psychology of the subject his work offers nothing new, no attempt being made to develop a genetic theory or to give a more intimate analysis of the conditions or expression of the sentiment, of its relation to pleasure-pain, to emotion and passion. The Spanish writer treats of it from the biological point of view as a functional expression of the tendency to adaptation, a form of originally useful reaction to the novel, and traces its evolution in animals, primitive man and children in its correspondence with mental development. The same point of view characterizes Ribot's study of antipathy (**16**), which is regarded as "an attenuated form of the instinct of conservation, acting by anticipation," and therefore as having a use in the preservation of the life of the individual, and, in its social forms, of the collective individuality of the group. Apart in this class of studies is Pfister's (**11**) examination by the Jung-Stekel method of free association and on the basis of the general assumptions of Freud's psychology of a case of suppressed hate and subsequent reconciliation, the subject and object of these feelings being respectively a boy of fourteen and his brother. The main conclusion about hate in such cases is that it gets imaginary satisfaction from images derived from the appropriate contents of actual or imagined experience, the injury desired being represented in the images as accomplished and the sexual component expressed in sadism or masochism. In reconciliation the earlier imaginations, possibly modified, are reinterpreted on the same or a sublimated functional level in accordance with the law of dreams.

Finally mention may be made of two papers dealing with certain pathological aspects of emotion. Hamilton (**5**), without resorting to artificial methods, such as dream-analysis, hypnosis, etc., learned from several of his patients facts which led to the conclusion that, in certain cases, neurasthenia begins in an unfavorable situation, or situations, not leading to satisfactory adjustment. His theory is that the affective tone of this state persists, giving rise to organic consequences made known to consciousness in terms of tension, and that the cardinal symptoms of neurasthenia—sense of fatigue, psychomotor inadequacy, etc.—are secondary effects of these organic consequences. Dugas and Moutier (**2**) discuss 'depersonalization' as essentially characterized by the loss or lowering of emotional tone, the exaggerated emotivity ascribed to certain subjects being, in either view, merely an expression of the romantic tendency to emotional make-believe. They distinguish it from an 'impersonalization,' in which emotion, owing to some organic defect, is habitually below the normal, as an emotional crisis, its factors being an emotion determining the

crisis, 'atony,' and the feeling secondarily aroused by its strangeness. While this last feeling causes distress, the atony itself is accompanied by repose. The detachment of the conscious states from the self cannot, in the opinion of our authors, be explained solely by the loss of emotion. The subjects have not lost personal consciousness, but a rupture has taken place between it and all of its states, the emotions appearing as illusory as the rest. The conclusion is that, while the loss of emotion is the characteristic and essential element which enables us best to comprehend the phenomenon, it only conducts us to the threshold of a mystery.

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ATTENTION AND INTEREST

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The recent work on attention includes two books of general character, one by Arnold (**2**) the other by Vaschide and Meunier. The first is a summary of the known facts of attention with many applications to educational practice. In addition to the matter indicated by the title it treats subjects of such prime interest for the teacher as fatigue and the nutritive value of foods. The standpoint is a combination of the sensory clearness view with the motor theory. Attention is at once sensory clearness and a sensori-motor response. The other volume (**8**) is also a summary of fact. The first half is devoted to the methods of measuring attention and a brief statement of results. Of the methods most emphasis is put upon reaction times. The new contribution of the volume is a study of the ability to waken at a desired time. Thirty-four people undertook the experiment and all succeeded; the average error was about twenty minutes, and the tendency was to waken too early. No very definite notion of the mechanism of the process is given. The final section is devoted to a criticism of the motor theory and the statement of the authors' conclusions that attention is primarily affective in character, that it is a dynamic process and of central origin.

The greater part of the experimental work of the year centers about Wirth's method of measuring the distribution of attention. O. Lipp (**6**) repeated Wirth's experiments, using practically the same apparatus and methods. In general the problem was to determine the limen for brightness in different parts of the visual field, under varying distribution of attention. The results confirm those obtained by Wirth except in a few details. The central and lower portions of the field are favored under natural attention. The influence of attention is not clear. The author refrains from attempting to interpret his results on this point. A third contribution to the Wirth-Geissler controversy is largely given over to the discussion of the degree to which the earlier criticisms were the result of misunderstanding. Geissler (**5**) takes occasion to admit the existence of individuals

who have a consciousness of the multi-level type as regards clearness. Geissler still holds to his opinion that Wirth's method is too complicated and that it fails of the desired end.

Whipple (10) puts to experimental test the statement of Miss Aiken that it is possible markedly to increase the range of consciousness by training. The method used was to test the number of isolated letters or objects that might be appreciated during an .08 second exposure. Each observer was trained by from one to two hundred exposures and then tested again. The amount of improvement was very slight and is explained by the author as due to general habituation or to the development of particular habits of forming groups. Very interesting results for the general problem of the range of attention were obtained by the use of a three and six seconds exposure. It was found that only six objects could be appreciated during six seconds as compared with from four to five in the .08 sec. This seems strikingly to confirm the conclusion that in the shorter intervals reading is from the memory after-image. These experiments with longer exposures show as little effect of training as the shorter.

Knight Dunlap (4) reopens the problem of the complication experiment and finds a new solution for it as a problem in visual reactions. He noticed in some early tests with the complication apparatus that the pointer is ordinarily clearly seen at the moment a disparate stimulus is given. This led him to believe that in the usual form of judgment the eye was following the pointer. When a net was introduced between the eye and the pointer, the eye could be kept on one point and the pointer was blurred at the moment of observation. It was found that there was no constant error. This suggested that the constant error in the ordinary or natural fixation method is due to a reaction of the eyes from the point fixated to the pointer. The displacement is really the expression of a visual reaction time. Three types of results are distinguished dependent upon the method employed: (1) the exact fixation method which gives no constant error; (2) the pointer following method which gives positive errors, since the eye does not come to rest until a little after the stimulus is given and the scale divisions are not noted until the eye is at rest; and (3) the natural reaction method which gives positive or negative errors according as the rhythmic reaction of the eye is anticipatory or delayed.

Billings and Shepard (3) publish an investigation of the effects of attention upon respiration and pulse by a more accurate method

than has generally been used. Each pulse was counted separately and plotted with the respiration. This made it possible to trace the effect upon the pulse rate of respiration as well as of attention itself. The results show that the different functions are very closely interrelated. In visual attention respiration is decreased in amplitude but the rate is sometimes quickened, sometimes slowed; in auditory attention the rate is always slowed but the effect upon amplitude is variable. The heart rate is increased by strained attention. Increased breathing, either in rate or amplitude, increases the heart rate; decreased breathing checks the heart. In consequence, the restricted respiration due to attention often gives a slower heart rate, particularly in the first moments of attending. This slowed rate is to be explained from the inhibited breathing rather than from a direct influence of attention.

A theoretical discussion of another experimental problem, the attention wave, is offered by Wallin (9). Wallin insists that the recent arguments against the existence of the attention wave, the result of experiments upon minimal stimuli, will not hold against the fluctuations of a reversible perspective. Here the fluctuations are independent of the strength of the stimulus. Wallin suggests that while part of the problem may be referred to eye movement, some remnant is probably due to circulatory or respiratory rhythms. He suggests, in addition, that the phenomenon is possibly related to the refractory phase of the reflex-arc. It is like it, in that the period of reduced activity is longer than the period of increased activity, and in that the period is different for the different senses. It is urged that the theory be put to an experimental test.

Two general discussions deal with paradoxes of attention. Titchener (7) answers Woodworth's argument, that you could not compare clearness with lack of clearness, because one can not compare without attention and attention would make clear the unclear and so destroy the problem, with the statement that it is a fact that you can appreciate and pass judgment upon unclear states. It is argued that clearness is related to judgment just as are intensity or duration. Intensities can be compared without changing the intensity of the weaker stimulus, and in the same way a clear and an unclear process when in consciousness together, touch off a judgment without undergoing any change in clearness.

Alexander (1) insists that will and attention are mutually exclusive. The train of ideas can not be controlled by will, for if one knew what the idea was that it was willed to bring to mind it would

already be there and the willing would be unnecessary. Similarly, holding attention voluntarily upon an object always defeats its end; either one is distracted from the object to the attending itself or too complete attending produces mental vacuity. Objects become clear only by constantly passing from them to related objects and back again.

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SPECIAL REVIEWS

MIND AND BODY

Der Einfluss psychischer Vorgänge auf den Körper. ERNST WEBER. Berlin: Julius Springer, 1910. Pp. 1–426.

As implied by the title of this work Weber's main thesis is that the vasomotor changes which occur in connection with psychical processes are merely accompanying phenomena, and not, as held by the James-Lange theory, causally related to the psychical processes. The significance of the vasomotor changes consists in the longer duration of the psychical process which they make possible, in rendering the execution of the activity corresponding to the content of the mental process easier and in regulating the intensity of the latter. In support o this view a large amount of critical discussion and first-hand experimental evidence is presented.

Because of the possibility of the very general application of the plethysmographic method, and the relative certainty and definiteness of its results, the author preferred to confine himself chiefly to a study of the changes in the volume of the blood supply in various portions of the body under varying psychical conditions. Records of other motor changes, especially those of breathing, were taken, but mainly as checks upon the changes in blood pressure and volume. Subjects were chosen whose breathing movements were not easily influenced by the conditions of the experiments, and in many cases disturbing factors were eliminated by the use of hypnotic suggestion. The latter method was also used as a means of influencing the psychical processes in the desired direction. The range of the investigation, together with the chief results, may be seen from a study of the following table, in which + indicates an increase in the blood supply to the part named, and − a decrease.

	Brain.	Ear.	Abdomen.	Limbs and External Trunk.
Movement ideas with or without performance	+	−	−	+
Mental work and increased attention	+	−	+	−
Involuntary attention and fright	+	−	+	−
Pleasure	+	+	−	+
Pain	−	−	+	−
Sleep	+		−	+

The opposite results occur under like conditions, in fatigue and pathological disturbances.

It will be seen from this table that Weber's main advance over the earlier work of this kind lies in his investigation of the conditions of the blood supply of the abdomen and of the outer portions of the head. The method used for the study of the latter was the fitting of a rubber capsule over the outer ears. The blood supply of the abdomen was investigated by the use of an 'inner plethysmograph' consisting of a rubber bag with tube attached. This bag was introduced into the lower intestine through the anus and slightly inflated. With the aid of an onkometer capsule a tracing was obtained showing changes in abdominal pressure, caused by variations in blood supply, due allowance being made for other factors.

A reference to the last two columns of the table will reveal the interesting fact that changes in the volume of the abdominal organs are always of an opposite character to those in the limbs and external trunk in the processes examined. This leads Weber to the conclusion that there is a reciprocal relationship between these two portions of the vascular system. He, therefore, believes the older view to be incorrect which held that there is reciprocal action between the cortical system of blood supply and that of the peripheral organs in such processes as increased attention and mental work. The apparent relationship is wholly accidental where it seems to hold and cannot apply at all in the case of agreeable and disagreeable feelings where the changes in brain and periphery are of the same general character.

Mosso's results with his tip-table cannot be explained as due to an increased volume of blood in the brain during mental work, for the brain being circumscribed by the skull, any increase in the blood supply must cause a withdrawal of cerebro-spinal fluid, leaving the net weight of the brain practically the same. The reason for the dip toward the head is rather the withdrawal of blood from the limbs and its influx into the abdomen, which in Mosso's experiments was placed mainly between the head and the center of gravity of the tilting system. Weber used a lengthened form of the tip-table and placed the subject upon it in such a way that the abdominal organs lay mainly between the feet and the center of gravity. Under these conditions the head end of the tilting board was raised rather than lowered during mental work.

Not only is there no necessary relationship between the cortical and peripheral blood supply but the two systems are governed by independent nerve centers. By stimulation of the appropriate nerves

in cats and dogs the author was able to vary the blood supply of the brain without any corresponding changes in the general blood supply taking place, and as these effects remained after destruction of the vasomotor center in the medulla, the conclusion was drawn that the center for the control of the cerebral blood supply lies centrally from the medulla. Furthermore, drugs such as alcohol were found to have different effects upon the brain from those in other parts of the body, thus confirming the supposition of their independent control. In these experiments both hemispheres participated in the changes in volume even when the nerves on only one side of the body were stimulated.

The relation of blood supply to movement was studied in two ways. In the first place, the motor cortex of cats, rabbits and dogs was electrically stimulated and the effect on the blood supply noted. It was found that such stimulation always brought about an influx of blood, but that the region of the brain in which the blood vessels were thus distended varied in the different species of animals. Simultaneously there was increased blood supply to the parts of the body concerned in the movement. In human subjects analogous results were obtained. Both in making actual movements and in thinking of movements, the volume of the brain and also of the arm or leg, to which the movement was referred, increased. The significance of this flow of blood to the brain lies, thinks Weber, in the replenishing of the cortical and muscular cells concerned in the movement, thus making possible the performance of the activity for a longer period and with greater ease.

It is not so easy to discover the significance of the changes in the distribution of the blood taking place in pleasure and pain. Weber believes that they are related to the processes of assimilation and dissimilation in both cortical and muscular cells. The constriction of the blood vessels in both regions under conditions of unpleasant stimulation leads to less rapid dissimilation and decreased sensitivity to the disagreeable stimulus, the rapid dissimilation having performed its function the moment the disagreeable feeling comes to consciousness.

The withdrawal of blood from the peripheral organs and its influx into the abdominal organs during mental work leads to a decreased sensitivity of the former, thus counteracting tendencies towards distraction of the attention. Cases are presented which show that this withdrawal is greater in difficult than in easy mental work.

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TIME AND FREE WILL

Time and Free Will. An Essay on the Immediate Data of Consciousness. HENRI BERGSON. Authorized translation by F. L. Pogson, M. A. New York: Macmillan, 1910. Pp. xxiii + 252.

The present volume is peculiarly difficult of detailed appraisal. It is not its special assertions so much as its whole point of view—its speculative technique, so to speak—that calls for first inspection: and, according to one's verdict about it, will Bergson's entire architecture be proved or discountenanced. What you think about introspection, you will think about Bergson; and, if you rate the method low, he has nothing to say which will better your opinion of it. The volume is an attempt to show, through introspective critique, the naturalness and also ineradicable error of every mechanistic, mathematical interpretation of mental life, including psychophysics. The first move is toward demonstrating that, contrary to supposition, psychic states have no quantity; and the first point to be made is that there is no such thing as *a* sensation with greater or less intensity; for intensity is not a disguised quantity, but a unique quality. If we remain loyal to the introspective method, we must say that there are as many different and irreducible sensation qualities as there are distinguishable intensities.

If this demonstration fails, Bergson's entire system must be revised; for it is the mainstay of his wider doctrines.

It becomes apparent, in the very first pages of the book, that Bergson has no notion of any other criterion of existence save that of awareness. "We call that space the greater," he says, "which contains the other. But how can a more intense sensation contain one of less intensity?" Now, this question plainly presupposes that, *if* the lesser intensity is contained in the larger, it must be felt *as so contained.* He says: "The question, then, is how we succeed in forming a series of this kind (like the cardinal numbers) with intensities, *which cannot be superposed on each other.* . . ." And, on the following page: "If we call that which contains the other the greater quantity, why go on speaking of quantity and magnitude, when there is no longer a container and a contained?" Can anything be plainer than Bergson's preconviction that the mathematical criterion is subject to the introspective? For, when he declares that there is, in *qualia,* no container and no contained, he assumes that if you do not sense the container and the contained *as such,* they do not exist. In short, Bergson's position is fundamentally Berkeley's. To his

way of thinking, it is a pure contradiction that a *quale* should increase in intensity without our immediately perceiving within it, absolutely unmodified, both the lesser and the greater intensity. To this we may retort that, after all, his first concern should have been to find some reason for accepting such a strange, unnatural test of reality.

The hypothesis has embarrassments, to all of which he seems quite oblivious.

In what does our perception of intensity exactly consist? Bergson's answer runs thus: "The more a given effort seems to us to increase, the greater is the number of muscles which contract in sympathy with it; . . . the apparent consciousness of a greater intensity of effort at a given point of the organism is reducible, in reality, to the perception of a larger surface of the body being affected." This is his general explanation of all 'apparent' intensities; the activity involved with the unextended psychic state radiates through the body, and a great mass of secondary induced *qualia* fuse in and around the primary *quale*, which fallaciously becomes, as it were, the subject of which they are attributes. But by what right does Bergson here reject his Berkeleyian criterion, which he has used to establish the difference between quantity and quality? We have seen above that he tests the existence of quantity by asking whether he can immediately perceive a container and a contained. Why does he not test the existence of intensity in the very same way? Why does he not say that, if you perceive a certain effect as a quantitative increment of another, this introspective evidence is conclusive of its being so? We directly experience one red as brighter, though still the same red; one flavor as sweeter, though still the same acid; one pleasure the more intense, though still the same quality. Why shall we pronounce this illusory, and yet hold that our perception is *not* deceptive when we fail to find a container and a contained? It is not enough to point out that the field of motor reaction enlarges and induces secondary sensations; for this proves absolutely nothing against the genuineness of intensity increments, inasmuch as these secondary sensations may somehow induce internal quantitative changes in the initiating quality. And there are many more such logical possibilities: as for instance, that the radiation and consequent induction of secondary sensations may be an effect of increased intensity in the initiating quality, precisely as the spread of a magnetic field and its heightened efficiency (measured in terms of any chosen unit) is induced by an intensification of an electric current. But, be the facts what they are—and we know precious little about them!—

over and above them towers the purely logical issue: why does Bergson here refuse the immediate testimony of introspection and appeal to a rare difficult sort, in his attempts to interpret all intensities as constellations of blending qualities, while, in the case of deciding the presence of quantity, he rests his whole case upon naïve impressions? His writings contain no answer.

The second, and, on the whole, the graver difficulty is this: Bergson's general hypothesis compels him to adopt a theory about the serial ordering of qualities which implies an unbelievable physiology. Consider the case of the tonal system. Are the differences in perceived pitch quantitative? No; the higher note is simply the one which taxes the more severely your tensor muscles of the vocal chords, when you attempt to produce it; and sometimes it is the note which resonates in your thorax, instead of in your head, as the high notes do. Once more let us waive an awkward question; this time about the way we know that tensor muscles are exerting a 'greater' or 'less' effort, and what this 'greater' or 'less' is (whether it is the pure number of induced qualities or a quality of the congeries itself). Let us consider only the general proposition that somehow the greater intensity, the higher pitch, the brighter light, the louder sound all owe their supposedly intensive character to the number and location of the induced sensations. What does this imply? Plainly that, just in proportion to the stability and definiteness of a quality-system, is the induced sensation congeries of each individual sensation fixed and constant. Thus, I am compelled to say that the tone C lies between B and D because C's induced fringe lies between those of B and D. *But how do I know that the fringes are so related?* What is there in one complex of supposedly pure qualities which forces me to judge that one must be interpolated between two others and *only between these particular ones?* Neither jointly nor severally can the pure qualities, as Bergson regards them, give this notion of betweenness, for each is completely non-spatial and non-quantitative. Bergson may explain each single intensity as being only a mass effect of a fringe; but this carries him not an inch toward explaining the rigid order of the tone series (and many others) *unless he also assumes that each fringe is invariable throughout a person's life and that he somehow senses the number or the patterning of the fringe elements.* The first assumption involves, as anticipated, an incredible view of physiology; the second assumption entails a belief in subconscious or else undefined perceptions,—a belief which, as we have seen, Bergson will not tolerate when it is a question of perceiving quantity but welcomes under other circumstances.

Bergson's mathematical prepossessions get the better of him in his exaggerated opposition between quality and quantity. Speaking as an introspective psychologist, he ought to say that perceived spaces are qualitatively different from one another. Now, in his chapter on Space and Homogeneity (p. 96 ff. especially), he recognizes this fact in animals. They, he asserts, sense directions as peculiar qualities. We have a vestige of this capability left us, in that we distinguish right from left by just such a natural feeling; "this," he adds, "is the very reason why we cannot give a proper definition of right and left." But the space which we construct out of non-spatial qualities is a "reality without quality," "an empty homogeneous medium," "a kind of reaction against that heterogeneity which is the very ground of our experience." We cannot hope here, to give this opinion the careful attention it deserves. Dogmatically, then, be it said that, in the reviewer's opinion, Bergson has not emancipated himself from a mathematical-geometrical convention, and with the result that his entire psychology and metaphysic are one prolonged struggle to explain a problem which, in reality, does not exist at all. The convention I allude to is that which asserts that 'pure space' consists in the absence of every quality. The meaning of this proposition evidently varies with the connotation of the term 'quality'; but, taking Bergson's own antithesis between quality and quantity, it means either that 'pure space' has no dimensions and no directions, or else that dimensions and directions are not qualities, but rather quantities! The absurdities of each alternative must be patent. We escape them, not by stripping space of all character, as Bergson does, but rather by conceiving it as actually having all directions and all dimensions. Instead of being immeasurably poorer than perceived space, it is infinitely richer; in it we can conceive differences of direction which perception can never yield, and dimensions beyond all the senses. It is, then, the perceived space which is the fragmentary cluster of qualities; as it were, loose tracts, chance streaks in real space which we sense.

The pervasive defect of these, Bergson's earlier studies, seems to root in psychological atomism, which is the very notion he means to oppose. This tendency is apparently connected with the very effort to attend to 'mental states'; perhaps it must condemn the introspective method altogether. If so, all of Bergson must be discarded. But, in any case, Bergson's introspection has landed him squarely in the old-fashioned doctrine of mental elements which can be brought into relation only by an act of the mind, which is precisely

and consciously the Kantian synthesis. "All unity is the unity of the simple act of the mind," says Bergson; and the mind has quite a chore getting the parts of such a unity together. "In order to get a number, we are compelled to fix our attention successively on each of the units of which it is compounded." Likewise, in time perception; "succession exists solely for a conscious spectator who keeps the past in mind and sets . . . symbols side by side in an auxiliary space." (In passing, we may observe how utterly different this Kantian psychology of Bergson's is from William James's 'radical empiricism,' whose feelings of 'and,' 'but,' 'along with,' and all the rest are precisely as immediate as reds and greens. And yet, these two philosophers honestly believed they agreed in their doctrines of percept and concept!)

It is a pity that Bergson has employed such means to found his profound hypothesis of real duration and indeterminism, which has actuated the writing of this book. That real time cannot be expressed in terms of space is a truth, the insistence upon which has made Bergson justly famous. Of equal importance is the corollary that the determining conditions of real action are immanent in the actual course of the action itself, and not capable of reduction to formal external factors. One of the great chapters of philosophy is the third in *Time and Free Will*, where Bergson beautifully demonstrates that to know completely the antecedents of an action is to be actually performing it, and that therefore the usual dilemma between determinism and indeterminism is meaningless, because each horn of it treats feelings and knowing processes as capable of being abstracted from the course of living conduct in which they arise and where they are genuinely efficient. To save the reality of space, in one manner or another, many philosophers have virtually abolished time. In this respect, oddly enough, Hegel and the ordinary natural scientist, with his mechanics of an 'empty' time, are alike. Bergson establishes the rich reality of time, but, alas! thinks it can be done only by virtually eviscerating space! Some day, perhaps, philosophers will not maltreat either.

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VAN BRUYSSEL, E. *Le mal et ses origines.* Paris: Giard & Brière, 1911. Pp. 216. 2 fr. 50.

BOHN, G. *La nouvelle psychologie animale.* Paris: Alcan, 1911. Pp. 200. 2 fr. 50.

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D'ERCOLE, P. *Il saggio di panlogica ovvero l'enciclopedia filosofica dell'hegeliano Pietro Ceretti, I, II.* Torino: Bocca, 1911. Pp. xvi + 513, 640. L. 12.

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PEREGRINUS, M. *Litora Aliena.* Boston: W. M. Leonard, 1911. Pp. 77. $0.50.

"JUSTUS." *Prolegomena to Theism.* New York: Andrew H. Kellogg Co., 1910. Pp. 70.

WELLS, F. L., & FORBES, A. *On Certain Electrical Processes in the Human Body and their Relation to Emotional Reactions.* (No. 16 of Archives of Psychology.) New York: The Science Press, 1911. Pp. 39.

NOTES AND NEWS

THE Eighth Meeting of Experimental Psychologists took place at Ithaca, April 17–19, on the invitation of the department at Cornell University.

THE New York Branch of the American Psychological Association met on April 24th in conjunction with the Section of Anthropology and Psychology of the New York Academy of Sciences.

THE First International Congress of Paidology will be held at Brussels during the third week of August. The announced object of the Congress is to coördinate, in the interests of education, the various results reached by the scientific study of the child.

THE following items are taken from the press:

DR. EDMUND B. HUEY has resigned his position as clinical psychologist to the Illinois state institution for the feebleminded at Lincoln, Illinois, to continue clinical research at the Johns Hopkins Hospital and in the city of Baltimore.

MISS LILIEN J. MARTIN has been advanced to a professorship of psychology at Stanford University.

EDITORIAL NOTE

THE editors of the PSYCHOLOGICAL REVIEW will welcome information as to omissions or inaccuracies in the List of Published Writings of William James which appeared in the March number of the REVIEW. It is requested that such information be addressed to Mr. Henry James, Jr., 95 Irving St., Cambridge, Mass. After sufficient time has elapsed to insure the completeness of the data the REVIEW will publish a supplementary list. Offprints of the bibliography have been sent to all members of the American Psychological Association. Those who are interested and have not received a copy may secure one by writing to Professor H. C. Warren.

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THE

PSYCHOLOGICAL BULLETIN

IMAGELESS THOUGHT: RÉSUMÉ AND CRITIQUE

BY PROFESSOR ROBERT MORRIS OGDEN

University of Tennessee

When Bühler published the conclusions which he saw fit to draw from his experimental analysis of the thought-process (**12**), he doubtless realized the storm of opposition which his views must meet. The storm has not yet abated, though it has taken a different turn from that of the first strong gust which issued from Wundt (**48, 49**) in his violent attack on the method which Bühler had used. In this respect, indeed, the cause appears to be making its most consistent progress. It is only necessary to survey the steadily increasing number of theses which have adopted the 'Würzburg Method' to be convinced that it has won recognition as a valuable addition to psychological procedure. As Michotte has well pointed out (**26**), if despite the incomplete character of the observations, one arrives at positive results, these results may be quite as valuable as any others. And at least with respect to the established importance of the 'determining tendencies' and 'tasks,' the method has justified its existence.

But the central contention of Bühler still remains an open question, and in America it appears to win adherents but slowly. Indeed, so strongly negative is the outcome of the investigations which have been undertaken in the laboratories of Cornell and Clark, where a large part of the recent work on this problem has been done, that it has seemed worth while, perhaps, for one who sympathizes with the 'imageless contents' to present a brief review and critique of some of the results thus far obtained.

By way of introduction, I may say a few words regarding the question of historical priority in this field. The credit which has

generally been accorded the Würzburg School in this innovation has been contested by Binet (7) who states that the 'method of Würzburg' were better named the 'method of Paris.' No one need doubt that Binet's results, published in 1903, were reached quite independently of the Würzburg work, but although it was not until 1907 that Bühler, in a sense, brought to a culmination the Würzburg investigations, Marbe had already used the method and published his results on the judgment in 1901 (24). Ach, too, who began his work in the Würzburg laboratory in 1900, claims the credit of priority not only for the method in its systematic form, but also for the 'determining tendency' and '*Bewusstheit*,' or imageless awareness of meaning (2, 18, 286, 9–10). The work of both Binet and the Würzburg people dates from the same year, 1900, but in Würzburg a more systematic method was employed from the start.

As for the results achieved, they had already been indicated in speculative literature considerably before this. James (19) in his *Principles* (1890) had advocated 'transitive parts' in the stream of thought, and he found in the writings of no less than five predecessors support for his belief in 'feelings of relation.' Similarly Stout (38) advocated the understanding of words without images through the mediation of 'psychic fringes' in 1896, while Ehrenfels' 'Gestaltqualitäten' (1890) (16) also play a rôle in the speculative origin of similar views of thought.

But we are here dealing with experimental results only, and should next refer to the independent findings of Woodworth (44, 45, 46, 47). In his study of voluntary movement (1903) he first became aware of the fact that a voluntary movement was not always nor typically called out by a kinesthetic image of the movement to be made. Later he took up this problem of the antecedent image with a larger number of observers. He did not anticipate the cases of no image at all, he writes me, but only the lack of kinesthetic images. "As the work progressed it became evident that the subjects were often unable to detect any image whatever; and so," he says, "I was led to the conclusion that one could have the thought of a movement without any image of it."

The experimental investigations which first pointed to the existence of imageless conscious contents have, then, at least three independent sources: Würzburg, Paris and New York, and the question of strict historical priority is of less importance than the fact that the discovery should have been made independently in each of these three places. In this connection, however, it is interesting to note that

each pioneer approached the question with a different problem in mind. The Würzburg work is the logical outcome of Külpe's emphasis in 1893 on the part that the preparation or disposition plays in all reaction experiments (**21**). This was again stated by him in an article on Attention which appeared in 1902 (**22**), but was written in 1899. These considerations doubtless paved the way for the future discovery of the 'determining tendency' and the 'task.' Regarding the character of thought as a distinct mental element, he tells me that the idea first came to him in the summer of 1898 while lecturing on Leibniz, when it occurred to him that the 'monads' were not concepts but *thoughts*. Binet's work, in turn, was the natural consequence of years of individual question and answer tests which he had employed with all manner of subjects for the purpose of characterizing and classifying individual types. Woodworth, finally, hit upon his discovery in the course of a study of the movement consciousness, as we have already noted.

So much, then, for the essential facts in the history of the subject. We come next to an examination of the results achieved. The work of the Würzburg School has been so thoroughly exploited in review and criticism, that I shall content myself with a bare enumeration of its principal phases. To Marbe (**24**) we owe the suggestive term *Bewusstseinslage*, the exact content of which is still, however, a matter of uncertainty. Originally used as a mere negative description for states of consciousness which were certainly present, but which seemed to defy closer analysis, the term received a further negative definition from Orth (**31**) who differentiated it from feeling. With Messer (**25**) it assumes direct relationship with thought, but Ach, who at first intimated its proximity to one of the forms of *Bewusstheit* (**1**, 236), has more recently declared that the two are entirely disparate (**2**, 9), since Marbe has denied to the *Bewusstseinslage* that which is characteristic of *Bewusstheit*, namely, the imageless knowing quality. The significance most commonly assigned to it consists in its identification with conscious attitudes.

To Ach (**1**) and Watt (**42**) we owe the careful experimentation which resulted in the more precise description of the important and far-reaching influence of the problem at hand and the determining tendencies which issue from it, as had already been foreshadowed in Külpe's writings. Ach also introduces the first experimental findings of this school for the existence of imageless knowing contents. The *Bewusstheit* is accompanied by sensations or memory images which stand as conscious symbols for the knowing quality. It is,

however, not reducible to these factors, for they may be present without this knowing quality. Ach also gives us a theory to explain the *Bewusstheit*. Every idea, he says, sets a number of other ideas which are associatively connected with it, in readiness for reproduction. This readiness for reproduction suffices for the *Bewusstheit*, without it being at all necessary for the associated ideas to appear in consciousness. The *Bewusstheit* is thus described as a developing function which represents a state of excitation among the reproductive tendencies (1, 219).

Kakise (20) in his study of understanding, at Clark, has reached a similar conclusion. His introspective reports suggest six stages in the understanding process: (1) pure feeling of familiarity, (2) pure feeling of concept or meaning, (3) pure feeling of content, (4) feeling of direction, (5) half developed images, (6) fully developed images. The first of these stages, being founded on past experience, is described as a 'retrospective quality' attaching to the sensations of the stimulus. The second is either a further development of this, or is identical with the third stage. It is here that we meet with the 'imageless thought' in its unequivocal form, and the author concludes, evidently by reference to his further stages of image development, that this stage consists in incipient excitations of a number of past experiences or images. This does not, in his opinion, yield a new element of thought, for thought of this character is neither elemental "nor a highly elevated something, but merely the most primitive and rudimentary form of reproduction" (20, 62).

We must note in connection with this interpretation that, whereas Ach insists that a complete arousal of the reproductions in question is quite unnecessary, Kakise concludes that "whether or not one has in the understanding of a word or phrase a concrete representation depends primarily upon the duration, *i. e.*, upon the time one dwells upon it" (20, 64). Kakise used two methods, an 'active' and a 'passive.' The first is the usual reaction method. He gives us no tabulation of his time results, but we may perhaps gather that they would not average over 1″ for word stimuli. Images were infrequent with this method. The 'passive' method, which he considers more effective, eliminates the reaction and makes the duration uniformly 3″. The results with this method show a preponderance of imagery.

The question as to whether the presence of rudimentary representative processes may be deemed adequate to the consciousness of understanding is basic to this whole problem. The advocates of the imageless content say that it is not; the opposition, in turn, says that

it is. It has never been denied, so far as I know, that some sort of imagery may be present at all times. It may be partially incipient, as Kakise describes it, and partially vestigial as Book (9), and Washburn (41) have shown. But the writers who claim that this is all we can know, and all that we need to know, concerning the pyschology of thought simply beg the whole question of the imageless content for the negative. Furthermore, the fact which Kakise adduces that increased duration is sufficient to bring the image clearly into focus, is, in my opinion, no argument against the imageless factors. With his 'passive' method he has simply made imagery the chief desideratum, as was also largely the case in Schwiete's (35) and Störring's (36, 37) investigations where the reaction times often run from 5″–20″. It is precisely in the brief moments of active thinking that the thought factor is most apparent. Whenever the constraint of a relatively quick reaction is removed, a premium is placed on discursive thought accompanied by a plentiful supply of words and images.

Coming back to our Würzburg workers, it remained for Bühler to find thoughts independent of any sensory factors, feelings or attitudes, elements which possessed neither sensory quality nor intensity, but manifested a degree of clearness, assurance and vividness. Bühler contends that images are too fragmentary and sporadic to be essential to thought. Thoughts occur without images as well as with them, nor does the thought become less clear as the image fades. As for the attitudes, they are not *actual* enough to cover the case. The consciousness of relation is too narrow a category, because, first, all consciousness of relation is not directed on images, and second, we often know the *what* of the thought before any image has developed. Bühler also notes that the laws which govern the thought-process are not identical with the laws of image association.

Binet's position is not so precise and clear-cut. He too finds his 'thought themes' inexplicable by the laws of automatic association. He notes the presence of concrete thoughts without imagery, also cases where the imagery is illustrative of but a small portion of the phenomenon. Often the image is not even coherent with the thought; one thinks one thing and images another. But on the side of interpretation, he is apparently not entirely convinced that his concrete thoughts are, as such, elemental. In his first work (5) he brings out the 'intention' as an explanatory category for them, that is, he emphasises the dynamic or functional phase of thought without clearly positing a static element. In a later work with Simon (6)

he examines the consciousness of certain aphasics and imbeciles, and concludes the presence of imageless thought because of the evident absence of verbal imagery in his subjects. He dismisses the possibility of other forms of imagery as constituting the real content of their thought processes, because, he says, it is impossible to imagine acts. He appears to have overlooked the manifold possibilities of kinesthesis in this connection (cf. Colvin (14)). His conclusion is that an intellectual *sentiment* or feeling gives us an advance taste of our thought; we thus perceive it before it becomes definite. In his discussion of current psychology for 1909 (8) he gives still another turn to his interpretation of the issue by identifying his 'intention' with 'attitude,' or 'tendencies of action which are arrested.' "The fundamental fact is the motor tendency." When this is inhibited we have the sense of attitude which is "probably but the psychic expression of corporeal attitudes which seize upon or are produced in us in a purely cerebral manner without leading to completely realised muscular contractions."

In turning to Woodworth, we shall not find it necessary to trace in detail the methods and results of the experiments noted in his four publications on this subject. These are all sufficiently well known to American readers. There is no question as to where he stands regarding imageless thought contents and their differentiation from images, but his work has possibly suffered for lack of more systematic treatment. The only other work on this subject, aside from Thorndike's utterances (39), which has issued from Columbia, namely, that of Betts (4), has been rather severely criticised (cf. Fernald (17)), and I fear justly, because of the author's failure to take into account the presence of verbal imagery in his subjects' reports. The experiments were performed on a large number of persons, and the lack of exact control is manifest in them.

Coming again to Woodworth, he as well as Bühler has focused attention on the adequacy of the imagery as a criterion for its essential connection with thought (44, 382 ff.). This point is criticized vigorously by Rowe in his recent study of voluntary movement (34). Rowe's first point is that with Bühler ruling out the image as being inadequate for thought, and Woodworth ruling it out as a cause of movement we have a *reductio ad absurdum*, since it is only necessary to carry the matter a step farther to be convinced that images are not essential to any of our mental processes. Rowe contends, second, that many things come in to modify the importance of the adequacy or inadequacy of the image in causing movement. Reflexes, instincts,

habits and the inflowing sensations are of vast importance, and need not at all be included in the image. Third, he argues that this idea of adequacy is not a psychological but a logical concept. It is a mere assumption that imagery to be adequate must contain what will be contained in the forthcoming movement.

Regarding the first point, it appears that the *reductio ad absurdum* is precisely what is indicated if one accepts the results achieved in favor of imageless thought. No one, I think, would quite wish to carry it to this extreme at once. That would be taking a rather broad jump at the conclusion. But Woodworth has spoken of imagery, generally, as 'by-play,' and in a specific instance as having "more the appearance of sparks struck off by thought in its progress than of thought itself" (45). The last figure of speech I find I have clearly imitated, though unconsciously, in my own conclusions on a specific case of introspected meaning (29). Even Pillsbury (32), discussing the topic theoretically, decides that imagery is subordinate to meaning, although he does not describe what the meaning content *is*, psychologically, seeming rather to indicate that the psychologist when he analyzes must content himself with images and leave the meanings to a somewhat different, broader and more concrete, aspect of the conscious process.

The second point refers only to the consciousness of voluntary movement, and finds its justification for Rowe in his results, which indicate that 'resident' and 'remote' images are alike superfluous to voluntary control. The movement, he says, is initiated by perception or 'the image of its natural stimulus,' and the control, instead of being effected by imaged or imageless thought, is entirely a matter of the immediate sensations and perceptions at hand. In short, the whole reduces to a continuous succession of motor adjustments. Just what determines an act as voluntary is the fact that it is 'cognitive.' The initiation of the movement is not the important thing, but the *control* which means cognition and is a sense perception of the end. That cognition should be equated with sense perception is apparently assumed as an incontrovertible postulate. The third point indicates this. We shall defer this question until the conclusion of our summary.

Let us consider, next, two other recent investigations on volition. Ach in his recent volume (2) gives as the result of his analysis of the act of will the following phenomenal content. The act consists in four 'moments': (1) the '*anschauliche*' moment (sensations of strain), (2) the '*gegenständliche*' moment (idea of aim and relation, means and ends), (3) the '*aktuelle*' moment (the activity '*ich will wirklich*'),

(4) the '*zuständliche*' moment (*Bewusstseinslage* of strain). The characteristic factor is the third of these. It is a unique element, being neither 'intentional,' nor a mere knowing about (*Bewusstheit*). No image factors are essential to it, though it is but a moment in a complex from which it can never be absolutely freed. What characterizes it as the essential feature of will is the fact that in its absence, although the other three moments may still be there, the character of volition is gone from the experience.

Michotte and Prüm (**27**), with a series of independent investigations in which a somewhat different method was employed, arrive at conclusions which substantiate Ach's main contention, namely, that a unique 'consciousness of activity' is essential to the mind in willing. For them, too, it is not a feeling of muscular activity, and is incapable of separate existence, being described, rather, as a simple 'modality' affecting certain phenomena so as to characterize them. Both investigators refer it to the *self*, and find in it an *actual*, not an *intentional* reference.

In reading these reports in connection with the work of Rowe, one is struck by the immense richness of content in the former, and the rather schematic poverty of the latter. Rowe has given us some interesting tests on the mechanics of volition, the real problem of its psychology he has missed entirely. And yet, he would probably retort that it is just the psychology which he has given, the rest with which Ach and Michotte deal is not psychology but philosophy. So here again we meet with the central question of all this work as to what psychology may be permitted to find in consciousness.

Let us deal next with a series of experiments on abstraction which have adduced important results bearing on imageless thought. First Külpe (**23**), in a report before the first German Congress for Experimental Psychology (1904), gave the outcome of some tentative investigations which date back to 1901. As a result he notes that the process of abstraction is based on apprehension and not on sensational differences, and he concludes from this that a distinction must be made between sensations and our perception of them: "that this distinction must be made in much the same sense in which we distinguish between physical phenomena and our consciousness of them; that, in other words, the old doctrine of an inner sense with the involved idea of a distinction between the reality of consciousness and objectivity must now have its opportune renewal in the domain of psychology."[1] Thus are we brought into close contact with the 'act and content' psychology of the Austrian School.

[1] Quoted from Moore, **28**, 92.

Grünbaum (**18**), working under Külpe's direction, brings renewed proof of this 'inner sense' in terms of the relation of sameness, a 'meaning moment' which his experiments reveal as an independent act which may be experienced without either member of the relation being apperceived. Furthermore, he finds this experience to be differentiated from the character of the members between which it operates, inasmuch as the same relation may exist with quite different relational bases.

Moore's work (**28**), which has recently appeared, was conducted partly at Leipzig and partly at the University of California. He too, substantiates the results of Külpe and Grünbaum. Perception, he describes as a "process of assimilating the data of sense experience to their appropriate mental categories." These mental categories are treated as the result of past experience presented as a supplement to the sensory data, but not reducible to either sensation, image or feeling. The principal criterion which Moore advances for the independence of this factor as an imageless thought or meaning, is contained in the act that any degree of certainty of recognition may accompany any degree of perfection in the percept. One may, perhaps, doubt if perfection in the percept is adequate to his contention. The test which he makes is that of ability to reproduce the figure which was recognized as having been repeated. This, to my mind, only substantiates a view which I have recently expressed (**29**), namely, that knowing and expressing are distinct processes. But this does not *of itself* prove that the knowing process is imageless. Many would contend that the physiological 'set' which is aroused by the perception is adequate to a judgment of certainty or uncertainty regarding recognition, without the exact contents of the percept being at once reproducible.[1]

The negative phase of Moore's criterion has more weight, in my estimation: the fact that his subjects at times knew all about the figure and were able to draw it from memory, yet were doubtful as to its being the common element of the series. But even here there is a possibility of confusing the content of these concepts with their functional value. So long as relational factors remain 'embedded,' to use Miss Calkins's term, in what are clearly sense perceptions, we shall find it difficult, I fear, to convince the opposition of their elemental character.

[1] Cf. in this connection an illustration by G. E. MÜLLER: Zur Analyse der Gedächtnistätigkeit und des Vorstellungsverlaufes. *Zsch. f. Psychol.* Ergbd. 5, 1911, pp. 139–141.

Passing over without comment, as we must for lack of space, the impor ant contributions of Bovet, first in substantiation of the general results obtained at Würzburg (10), and more recently, in contributing to the differentiation of the attitudes involved in the sense of duty, or oughtness (11), we must proceed finally to a consideration of the work of Titchener and his school. Titchener has been the one American psychologist who has given most attention to the phase of experimental psychology which we are reviewing. Not only has he expressed his views at length in a series of lectures (40), but he has also directed a number of his students in experimentation with the Würzburg method.

Titchener's position, as is well known, is negative. He confesses to the 'sensationalistic bias.' In putting ourselves in touch with the reports from his laboratory, we shall do well to understand first the nature of his antagonism to the new thought element. Being of a richly imaginative type, he does not find himself among the 'tailless foxes' of Winch's fable (43). Neither does he see any reason for granting the existence of foxes which have not at least a vestigial caudal appendage. Consequently, the prime question for him is: *do images mean?* If they do, why accept a new element for meaning? In substantiating his conviction that images are the only meanings which psychology can adduce, he declares that meaning must be taken *sub specie existentiæ;* all else is logic. It is the confusion of logic with psychology which brings Bühler to the statement that "it is impossible to ideate a meaning; we can only know it." The natural consequence of this confusion is, for Titchener, the 'act and content' doctrine of Brentano and the Austrian School, which separates the act of knowing from the content known. This doctrine he has accordingly subjected to a somewhat lengthy destructive argument. Regarding this doctrine as the natural consequence of imageless thought, if I may be permitted to refer again to my own distinction of the knowing and expressing phases of consciousness, I should like to submit that it is not impossible to conceive thought and knowing from the point of view of mental content, as existing in direct causal relationship with the expressive consequents which are sensational and imaginal. But Titchener would doubtless contend that in this case the antecedents are of the same quality as the consequents. This, however, is the point at issue, and dogmatic assertion on either side will never settle the outcome. We must turn, then, to Titchener's criticism of the findings of imageless thought. The findings, he says, are not convincing, because the observers were all under the

influence of the 'stimulus-error' in their introspective reports, that is, they have reported *about* their experiences, without really describing the contents which they experienced. This is much the same criticism which had previously been uttered by Dürr (15) and von Aster (3).

Now, it has not been denied that '*Kundgabe*' is mixed with all introspective reports of this nature. But if the error be regarded so serious as to discredit the entire result of Bühler's investigation, as Titchener appears to believe, may we not carry the point a step farther and deny the value of *all* introspection? Indeed, in a recent discussion among psychologists, this position was vigorously maintained by two among those present. We cannot go into this question here, but assuming that introspection possesses at least a relative value, as I believe would be commonly accepted, I am impressed by a counter-error in some reports on the higher mental processes which is equally insidious to that of *Kundgabe*. At least the latter, if the observer be reliable, can be counted on to *intimate* what was going on in the mind at the time of the experiment. The other error to which I refer confines introspection to the known mental categories of sensation, image and feeling in which the observers have been schooled.

A colleague in a neighboring institution who is conducting some investigations on the learning process, reported recently that in his own laboratory, with relatively untrained subjects, imageless thoughts were plentiful in the introspective reports. When he transferred his work to one of our leading laboratories and repeated the experiment with 'trained' observers, nothing was reported but sensations, images and feelings. At first glance this would seem to indicate that the untrained observers had failed to analyze their experiences, and this may well have been the case in a certain degree. But it may also indicate a predisposition on the part of the 'trained' observer which is decidedly prejudicial to the discovery of *meanings* in experience. I seem to detect this predisposition in all the introspections which the Cornell investigators have put forth.

Pyle in his study of expectation (33) concludes that this conscious attitude consists in organic and kinesthetic sensations depending on a psychophysical 'set.' No 'knowing' factor is registered by his observers, and the difference between definite and indefinite expectation is regarded as entirely a matter of the 'situation'; it is not *known* at the time it is experienced.

Okabe in his study of belief (30) gives us results which are simi-

larly external to actual meaning. He finds, it appears, no difference between the consciousness of belief and the consciousness of disbelief. Both are 'consciousnesses of the same kind,' consisting in sensations and images, and occasionally, though not necessarily, emotional. One might imagine that Okabe's results would apply equally well to a description of the esthetic attitude, the ethical attitude, the consciousness of understanding, or indeed any other of the higher apperceptive states of mind.

His improvement of the method of introspection to the extent of confronting each of his subjects at the end of the investigation with a tabulation of his utterances, does not seem to have accomplished more than to elicit from each obsever the assurance that the tabulation was correct. Had alterations been suggested here and there, the improvement might have proven its worth; as it happened, it appears to have been quite a perfunctory affair. This same procedure was followed by Clarke in her work on conscious attitudes (13), and with the same equivocal result.

Clarke's analyses bring clearly into focus the point which I have been making. Her subjects lay all stress on the sensory factors in their experiences, and whenever meaning factors appear, they are either passed over by the author, or referred to as *Kundgabe*. It is interesting to note, however, that in the analyses given of some sixty attitudes such as approval, caution, difficulty, surprise, etc., there appear several which, as named, clearly involve an element of *knowing*. For instance, the 'consciousness that the letter was too small,' 'fear that I had reacted too quickly,' 'I ought to know that,' etc. Yet each of these is unblushingly analyzed into 'muscular strain and organic sensation,' 'slight sinking of the stomach and diaphragm,' and the like. The author does not make it quite clear whether she would have us understand that an attitude such as 'I ought to know that' is really equivalent to 'organic sensation and disagreeable feeling,' or whether the *meaning* of the attitude is a transcendental factor. I find Titchener, himself, somewhat unclear on this point. If, as he says, meaning is always context, and context is presented as a group of sensations and images, this would seem to imply a very elaborate system of symbols in terms of imagery and kinesthesis to fill all the demands of the knowing attitudes. The difficulties met by the James-Lange theory of emotion in equating organic complexes with diverse affective experiences,[1] suggests the far greater difficulty which must

[1] For a brief statement of these difficulties, cf. TITCHENER, E. B. *A Text-Book of Psychology*. New York, 1910, p. 477.

arise in equating the manifold possibilities of thought with groups of sensations and images. The one thing which introspection has revealed with some degree of certainty, is the fact that the same thought or relational experience may be present in the same as well as in different minds, with quite variable sensational and imaginal accompaniments.

The other alternative is to shift meaning to the realm of the transcendental or to the unconscious, and I submit that either course is simply to avoid the issue. The consciousness of meaning is a consciousness which we all experience; whether it can or cannot be analyzed is another matter. But the problem is not materially helped, so far as I can see, by denying its psychological existence.

It is evidently under the assumption that images may *carry* thought, without *being* it, that Clarke concludes from her experiments that the imagery need not be specific or elaborate for this purpose. As for inadequacy of the image, we have, she says, no criterion to tell us how "clear or complete an image must be in order to carry a meaning." In repeating experiments with Woodworth's 'rule of three' method, she finds "not a single case of a relation being consciously carried in non-sensory terms." Any yet, I have a feeling that the following experiment, at least, would have been interpreted somewhat differently by Woodworth: "Red : blue :: green : *yellow*. I started to say this automatically. Then I repeated the stimulus and said 'intermediate' verbally. *Some kind of consciousness that meant 'principal colors.' I did not say 'principal.'*"[1]

Thus we see that it is the fundamental problem concerning the psychology of meaning which directs as a powerful determining tendency the outcome of all these experiments on the content of thought. Those of us who believe that meaning is a conscious factor, directly given in our experience, find it unequivocally described in our introspective data, despite all the errors of *Kundgabe* which may creep in. Those, on the other hand, who believe that meaning is a logical concept which can be psychologically observed only in terms of sensory 'vehicles,' obliterate the *Kundgabe* from their reports and direct attention on the sensations and images which may be present. The experiments of the latter group have been chiefly negative, that is, they fail, as of course they must fail under such an 'Aufgabe,' to find the imageless content. But positively, I cannot see that they give much promise of being able to work out a psychology of thought in terms of sensory symbols.

[1] *L. c.*, p. 243. The italics are mine.

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PSYCHOPHYSICAL MEASUREMENT METHODS

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The activity in this field of work within the last two years contrasts curiously with the indifference with which the problems of psychophysics were treated formerly. The number of papers mentioned in this report is considerably greater than that of the papers enumerated in the PSYCHOLOGICAL INDEX for the preceding years. It is convenient to begin the presentation with the new edition of Wundt's *Principles of Physiological Psychology* (10). The chapter on psychophysics was considerably enlarged and thoroughly overhauled, but the characteristic features of the views advanced in the previous editions are unmistakable. The view that psychophysics is not an independent science, but only the helpmate of psychology, is strongly accentuated. Psychological observations made incidentally to psychophysical experiments are sometimes of greater importance than the direct outcome of the experimentation. Wundt's criticism of the values derived from the statistical treatment of long series of experiments or of experiments on large groups of individuals are of great interest. The values obtained in psychophysical experiments differ from those derived from physical measurement also in this, that their variations are not due to accidental errors but to variations of the quantity determined. By combining results from different periods of time or from different stages of training one is liable to obliterate the traces of factors which are of the greatest importance, and to obtain results void of psychological significance. The presentation of the methods of minimal changes, of mean gradation and of right and wrong cases has not been changed materially, but the chapter on the methods of counting off contains some discussions about the curves of distribution, a short criticism of the method of ideal ranges (*Idealgebiete*), and a presentation of the method of multiple cases, for which the material of Keller's acumetric experiments is used.

William Brown (1) published a study on the use of the theory of correlation in psychology, which is all the more significant since it carries with it the authority of Pearson. In the first part the correlation coefficient, the correlation ratio, the probable errors, and multiple correlation are explained. The author then proceeds to discuss other methods of determining correlation and mentions

Pearson's criticism of Spearman's work. Then follows an outline of the history of the theory of correlation as applied to psychology. The last part contains the description of a series of tests which were made on fairly large and homogeneous groups of students. The tests comprised crossing out letters, bisecting and trisecting lines, the speed of addition, the Müller-Lyer and the vertical-horizontal illusion, memory, combination, association and general intelligence, marks for drawing and general school marks. The correlation is low and the groups of coefficients show no hierarchical arrangement, which to some extent bears out Thorndike's results. Spearman's formulæ are found not to give satisfactory results.

The paper mentioned under (2) is a reprint of the last chapter of the author's doctor's thesis.

The third paper of William Brown (3) contains the study of one of the more complicated mental functions. A group of 83 students was examined in geometry, algebra and arithmetic. The papers were first marked according to the ordinary school standard, and then according to a differential system of marking based on the analysis of the mental processes involved in answering the questions. The correlation between algebra and arithmetic is greatest, then come geometry and arithmetic and last geometry and algebra. The author infers that geometrical ability is only related to algebraical ability through the mediation of arithmetical ability. The result that memory of preceding propositions is most closely related to the other forms of geometrical ability, is a similarly interesting result. The author proposes to test in a future publication the existence of a general mathematical ability or a general geometrical ability. For this investigation new formulæ will have to be devised, since those in existence are open to criticism.

Warner Brown (4) took up the analysis of the notion of the threshold of difference along the lines pointed out by Peirce and Jastrow. On the basis of 75,100 experiments on lifted weights he finds that all differences, no matter how large they are, are misjudged a certain number of times if a sufficient number of experiments is made, and all differences, no matter how small they are, are judged correctly in a certain number of cases, the percentage of right judgments increasing when the difference between the stimuli increases. From this he concludes that there does not exist a threshold of difference, if this word designates a difference which is always judged correctly, nor has it any meaning to speak of the threshold as of a difference which is so small that it

always passes unnoticed. There remains the possibility of defining the threshold in terms of the relative frequency of correct judgments, but Brown does not see any reason why one value should be preferred to any other. The author certainly has the merit of expounding Peirce and Jastrow's ideas and of illustrating them by means of a large experimental material. A number of investigators, however, think that this criticism of the notion of a threshold is rather out of date. A distinct addition to our knowledge is Brown's report on an investigation, as to whether the form in which the judgment is expressed influences this judgment itself. He made series of experiments in which all the conditions, except the way in which the judgments was expressed, remained constant and showed that the relative frequencies of correct judgments in series with oral expression of the judgment and in those with motor expression were not the same. These differences are attributed to some bias or mental defect of the subject.[1]

It becomes acknowledged more and more that an original treatment of the problems of psychophysics cannot be given without an adequate knowledge of certain branches of mathematics, although the practical applications require only very little mathematical ability. The theorems, which are needed in psychophysics, are not given in most of the text-books and the original treatises are not easily accessible to psychologists. The second edition of Czuber's well-known book on the calculus of probabilites (5) conveniently supplies this need and will be found a great help to the student. The following chapters will be of special interest to psychologists: The series of Bruns for the measurement of groups, four place tables of the probability integral and of its derivatives, and a short presentation of Pearson's views. Bruns's theory of the measurement of groups has found only little attention, partly because the author spoke in his book as a mathematician to mathematicians. It is to be hoped that Czuber's clear presentation will do away with this difficulty and that the value of Bruns's series for the problems of psychophysics may get a fair test.

The present writer (6) published a study of the method of multiple cases. The distinctive feature of this method is the admission of five different judgments. The notions which served for the analysis of the data of experiments with three judgments are sufficient also for the analysis of this method. The notions of the probability of a judgment and of the psychometric functions are fundamental.

[1] See Special Review on p. 218.

First a method is given by which the observed relative frequencies of the five judgments can be adjusted and it is seen that the calculated and the observed values coincide remarkably well. Then the theory of ideal ranges is taken up and the conditions are found under which these qualities may be used as measures of sensitivity. The results of Keller's acumetric experiments show a rather close agreement between the interval of uncertainty and the ideal range of the middle judgments, so that one has to conclude that the above-mentioned condition is fulfilled. It remains to be seen whether experiments with other stimuli will give similar results. The last part of the paper contains a criticism of a method for the determination of the sensitivity, which was suggested by Ebbinghaus. The results obtained by this method are in no relation to the values which are found for the interval of uncertainty.

A short presentation of the theory of psychometric functions was given in the paper on the method of constant stimuli (**7**). One has to distinguish between the treatment of the results by indifferent formulæ (such as Lagrange's or Newton's formulæ of interpolation) and the adjustment according to a definite hypothesis on the psychometric functions. Each procedure has its own advantages. We must be guided by experience in the choice of a definite hypothesis on the psychometric functions and find out which hypothesis fits the given data best. Two such hypotheses are tested and it is shown in detail how the relative value of an hypothesis must be judged.

Keller published some time ago the results of a very extended series of acumetric experiments without giving an adequate discussion of his material. The results of an adjustment of these data according to the so-called $\Phi(\gamma)$-hypothesis was given by the present writer (**8**). It may be of general interest to psychologists, that the time spent in working out the data is very small when compared with the amount of time necessary for obtaining them, provided that the work is done systematically. The proper treatment of good experimental data requires less mathematical ability and is less laborious than is commonly believed.

Wirth's paper (**9**) on the direct treatment of the data of psychophysical experiments is one of the most significant productions of the last year. It corresponds to the treatment of the psychometric functions by an indifferent hypothesis and has a very wide range of application. The salient feature of the paper is the restriction to a range outside of which one of the psychometric functions of the extreme judgments has the value *one*. This is a new idea, because

until now the psychometric functions of the extreme judgments were always represented as asymptotically approaching the values *zero* and *one*. Starting from this idea the author develops formulæ for the threshold, for its mean variation and for the mean square of the deviation. Some of these expressions are rather complicated, but Wirth gives formulæ of approximation, by which the calculation requires an insignificant amount of work. The usefulness of this article is increased by numerous examples, with which the application of the formulæ is illustrated. There is no doubt that Wirth's ideas will prove useful also in other fields of work. The realm of psychophysical measurement methods to-day is not as restricted as it was in Fechner's time. One may say that it reaches as far as the method of experimental variation of the conditions. In some fields of work, as for example in the investigation of higher mental processes, it is impossible to collect a very extended experimental material and it is exactly in these cases that Wirth's method will prove useful.

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REPORTS OF MEETINGS

EIGHTH ANNUAL MEETING OF EXPERIMENTAL PSYCHOLOGISTS

Experimental psychologists from Clark, Dartmouth, Harvard, Hobart, Johns Hopkins, Mount Holyoke, Princeton, Tennessee, Vineland, Wesleyan and Yale gathered for their eighth annual meeting at the Psychological Laboratory of Cornell University, Monday to Wednesday, April 17–19. Among the visitors was Professor G. Tschelpanow, of the University of Moscow, who is studying American laboratories in order to perfect plans for the erection and equipment of a new laboratory building in the Russian university which he represents.

Monday morning was spent in an inspection of the Cornell laboratories; special interest was shown in the demonstrational apparatus. The afternoon meeting opened with a paper by Bentley (Cornell) on 'Comparative Psychology and the Naturalist.' The reader pointed out the essential differences between the appreciative or sentimental attitude toward animate nature, and the scientific attitude whose purpose is accurate description and comprehension; he urged that, in order to the study of the animal consciousness, the field-observations of animal behavior made by the naturalist must be supplemented by psychological experiments, designed to show, *e. g.*, what sense-departments play a decisive rôle in the animal's conduct and how the creature solves problems presented to it by typical situations. Porter (Clark) reported an extended series of observations carried out by L. W. Sackett on the porcupine, which showed that in certain traits of intelligence this animal ranks between the raccoon and the monkey,—a result which contradicts the popular notion of the porcupine's stupidity. In the discussion aroused by these two papers, the question came up whether or not, from the point of view of university economy, it would be advisable to affiliate experimental psychology to the biological sciences. It developed that, in many universities and colleges, experimental psychology is still in a transitional period: the majority of those present believed that the subject should be placed on the same level with, but should remain independent of, the physical and biological sciences.

The topic of the evening's discussion was 'The Methods of Psychology.' In coöperation with the members of the graduate seminary, Jacobson (Cornell) had prepared in tabular form an empirical statement of psychological methods as advocated and employed in current text-books, and Foster (Cornell) had reduced this empirical list to a logical table, which he presented and explained. The subject was discussed further by Dodge (Wesleyan), Holt (Harvard), Sanford and Porter (Clark), Titchener and Whipple (Cornell).

On Tuesday morning, Dodge demonstrated by means of lantern slides the operation of a highly sensitive recording device of low latency. It is based upon the principle of photographing, upon a falling sensitive plate, the projected shadows of a lever which records highly magnified movements of vibrating forks, of membranes indicating pulse or breathing movement or change of volume, and of muscles concerned in such reflexes as the wink and knee-jerk. Ogden (Tennessee) reported experiments on images of memory and imagination, which were designed to test the results obtained by Perky. In experiments on 'imageless thought,' he measured separately the reaction-times for the appearance of meaning, and of visual, auditory and kinæsthetic images, and found that the meanings often arose as much as 0.5 sec. more quickly than the imagery. The subsequent discussion brought out a number of interesting observations with regard to the various types of kinæsthetic images, and the translation of meanings into attitudinal representations.

At the afternoon meeting, which was held in the psycho-educational laboratory, Johnston (Johns Hopkins) outlined the research work in progress at Baltimore. Experiments on the most economical method of learning in animals showed that the distribution of trials into daily groups was most effective; after an interval of 60 days scarcely anything had been forgotten. The results of Watson's experiments on white rats proved that these animals have practically no color vision. Watson's new circular maze, with various standard adjustments, was described as practically completed. Johnston's own work on the sensory equipment of dogs revealed the fact that the animals do not rely to any great extent on vision, especially in novel situations. McComas (Princeton) gave an account of tachistoscopic experiments on types of attention, in which he had found a high degree of correlation between attention-span and association-times. Angier (Yale) raised the question whether it is possible and advisable to correlate the credit-marks of college students with some

other and more objective tests of their standing and intelligence. The discussion brought up a number of practical problems, which are awaiting their solution at the hands of an applied psychology. While some of those present found in the use of correlations a promising method for determining, *e. g.*, what chances a given individual has of success in a given occupation, others expressed doubt on the ground that a few experimental tests in a restricted field could not do justice to the complex problems presented by the occupations of everyday life. The significance and applicability of the method was later demonstrated by Goddard (N. J. Training School, Vineland) in the case of 400 backward and feeble-minded children, whose intelligence he had studied by the Binet-Simon tests and had compared with the intelligence of 2000 normal children. He "obtained figures of distribution which agreed so amazingly with the curve of normal distribution that there can be no doubt that the Binet tests are marvelously accurate." His tables of comparison, relating a child's intelligence as measured by the Binet tests with its intelligence as expressed by the difference between age and school standing, furnished further evidence of the usefulness of these tests.

At an informal meeting in the evening, Porter (Clark) exhibited a novel apparatus for mapping the blind spot, and Baird (Clark) showed a laboratory stand, of cheap construction, which nevertheless permits of fine vertical adjustments.

The Wednesday session was opened by Langfeld (Harvard) with a report of association experiments, involving suppression or negative instruction, under the influence of alcohol and caffeine. With 30 c.c. of alcohol, the association-time averaged 0.1 sec. faster, the reproduction-time 0.2 to 0.5 sec. faster than the normal, while 6 gr. of caffeine reduced the association-time by 0.2 sec., and had practically no influence upon time of reproduction. Introspection revealed great individual differences of attitude towards the instruction. Similar experiments, in cases of dementia præcox and manic-depressive insanity, showed in the former a high percentage of failure to suppress, in the latter a practically normal behavior. Ford (Harvard) described a new method for obtaining judgments of time-reversal; electric shocks are given, under distraction, to the two wrists at intervals varying from 1/10 to 1/80 sec. Bingham (Dartmouth) reported the results of a statistical inquiry into the minimal cost of installing and maintaining a college laboratory.

Angier reported from the Yale laboratory experiments on visual contrast with subliminal color-stimuli, and mentioned among others a

study of æsthetic symmetry, in which the influence of bilateral motor processes appeared to be essential. Cameron (Yale), studying reversible perspective, found that eye-movements are not, as is frequently supposed, necessary to this form of optical illusion. Baird outlined the results of three researches conducted at Clark University. The first is an investigation of the relations obtaining among the various taste-qualities; the results differ materially from those of Kiesow and Oehrwall. The second is a study of the organic effects of musical enjoyment and appreciation; characteristically different curves and introspective records are obtained from two types of hearer, the passive or receptive and the intellectual or critical. The subject of the third study is the stammering child. Pneumographic records show a lack of coördination of thoracic and abdominal breathing.

Tschelpanow then addressed the audience in German, describing the status of experimental psychology in the Russian universities. The progress of the science has been impeded partly by changing political conditions, partly by the close connection of psychology with philology, and partly also by the fact that students in the universities are in fact only of collegiate rank, and still go to Western Europe to complete their psychological education. Recently a business man of Moscow, Mr. Shtchukin, well-known as a patron of the modern school of painting and owner of a large gallery of modern pictures, has given 100,000 roubles ($50,000) for the erection of a laboratory building, and 20,000 roubles for its equipment. The new laboratory is also endowed with a library of 3000 volumes, presented as a memorial by the family of a young instructor who met with an untimely death. Tschelpanow is to be the director of this laboratory.[1] Applied psychology has of late become influential in Russia, owing to the work of Netschajeff and his collaborators in the *gymnasia* and other secondary schools, where psychological instruction has been introduced. Interest in physiological psychology is sustained by the familiar names of Bechterew and Pawlow.

Smith (Toronto) reported that the laboratory of his university is in a state of reorganization; the present year has been profitable chiefly on the side of undergraduate work. Edwards (Cornell) described his experiments on suggestion, giving particulars of method and some preliminary results. The meeting was brought to an end with Bentley's report of two studies recently conducted at Cornell. The first is concerned with the factors that determine choice under

[1] A more detailed account of this address appeared in *Science*, May 26, 1911.

balanced motives. Its two aims are to get at the influences operative in autosuggestion and social suggestion, and to analyze the judgment which issues from two motives of approximately equal strength. The second study enquires into the dependence of thermal intensity upon area of stimulus. The dependence appears only when more highly tuned temperature-organs are involved, and when conductivity by the skin is enhanced; no summational effects are found either for cold or for heat.

It was decided that the ninth annual meeting should be held at Clark University.

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THE INNSBRUCK CONGRESS OF 1910

Bericht über den IV. Kongress für experimentelle Psychologie in Innsbruck vom 19 bis 22 April 1910. Von F. SCHUMANN. Leipzig: Barth, 1910. Pp. xxviii + 312.

Again the German experimentalists present a substantial volume as the product of their annual meeting. The number of communications and the summary character of many of them make any thorough résumé of the volume out of the question. The most that can be done is to select a few papers for summary. The contributions of the volume consist of four relatively long reviews of literature in different fields and thirty-five shorter abstracts of papers, with a description in a third part of the apparatus shown at the congress.

The first of the papers is by v. Monakow on the development and localization of movements in man. It is insisted that to understand movements it is necessary to analyze them into the components that are derived from different phylogenetic ages, as well as into the segmental localization, their focal localization, according to their end and occasion and their mental accompaniments and antecedents. Each of these factors must be taken into consideration in any act, and in any act each contributes in very complicated fashion. No movement can be ascribed to any one part of the cortex alone, nor to the cortex to the exclusion of the lower centers and the cord. The details of the treatment can not be gone into here, but the paper gives an excellent summary of recent work on localization of function.

The second paper by M. Geiger gives a statement of the *Einfühlung* theory with arguments for and against different phases of theories and possible theories. An idea of the wide range of subjects

treated may be given by the four main heads which are: A, the understanding of foreign expressive movements; B, the understanding of foreign personalities; C, the personification (*Beseelung*) of the subhuman, and D, æsthetic pleasure and *Einfühlung*. Authors are cited from Hume to the present. No particular conclusion is reached that can be stated within our limits, but the work takes an authoritative character from the author's close relations with Lipps, and is a compendious summary of opinions on the topic.

Alexander gives a summary of recent work on the functions of the vestibular apparatus. A brief history of fact and theory results in the conclusion that the Mach-Breuer theory is entirely adequate, particularly for the functions of the semicircular canals. The difficulties in the investigation of the sensory functions of the canals arise from three characteristics: (1) while the other sense organs have distinct functions, and qualities that do not pass over into each other by slight gradations, the vestibule has no function that may not also be performed by eye, or by some one of the tactual-kinæsthetic group; (2) the nerves of the labyrinth do not make direct connections with the cortex, but their activity can be known only through the reflexes that they excite; and (3) while the other senses only change the amount of stimulation in relatively slight degree in pathological conditions, the vestibular organs undergo a change that is altogether different, quantitatively and qualitatively, from the normal. It is doubtful if appreciation of the vertical may be ascribed to the vestibular apparatus, but it has an important function in aiding in the perception of the movement of different members of the body and in the appreciation of weight and resistance. This points to the tonic influence of the labyrinth as highly important. A full discussion of the reflexes excited by rotation is given, including means of distinguishing between the nystagmus and dizziness of vestibular origin and those of optical and pathological origin. Even more interesting because less well known is the discussion of the thermal, mechanical, and galvanic methods of exciting reflexes from these organs. They are important because it is possible to excite an organ on one side of the head and leave the other unaffected. Irrigating the ear with water above or below the body temperature will produce characteristic but opposed nystagmus and accompaniments. The same effects may be induced by changing the intralabyrinthine pressure. This stimulus can be applied only in a few individuals who have unusual movability of the windows or in case of labarynth fistula. Galvanic simulation of the ear with the kathode on the ear produces nystagmus

toward the kathode. On opening the circuit the direction of the nystagmus changes. Much less is known of the functions of the sacculus and utriculus. The reflexes that may be ascribed to them are (1) the compensatory eye-movements, (2) reflex head and body movements, and (3) the maintenance of equilibrium. It seems probable that the semicircular canals affect the tonus of the eye-muscles while the vestibular apparatus is of more influence on trunk and limbs. The functions of this part are much less well understood than are those of the semicircular canals.

The longest, and to an experimentalist probably the most useful, of the general reviews is by Ranschburg of the results of the experimental psychopathology of memory. It brings together an immense amount of material in every field and covers the ground from the theories of Freud to the most technical of the methods of diagnosis of the feeble-minded and insane. He is by no means ready to grant all of the claims of Freud and Jung as to the mechanism of forgetting or even as to the facts upon which the theories are founded. He adduces the work of Schnitzler as evidence that it is not yet assured that emotionally toned ideas will always give delayed reactions. Schnitzler's results seem to cast doubt upon each of the fundamental laws of Jung. Emotionally toned ideas do not always give slow reactions, in fact it is possible that the reaction is quickened, and there is no evidence of general delay in the post-critical reaction. He denies also that it is assured that the unpleasant tends to be forgotten. The work of Gordon and Külpe gave no evidence of an effect of feeling of any sort upon the course of retention and Kowalewsky and others have found that in almost 70 per cent. of children the disagreeable is more likely to be retained than the pleasant or the indifferent. His own studies, among others, convince him, too, that the forgetting of every-day life which Freud ascribes to the oblivescence of the disagreeable is due rather to the various forms of inhibitions that Müller and Schumann and Müller and Pilzecker established, particularly to effectual inhibition. Evidence of this is found in the similarity to others of the names that are usually forgotten, and in the fact that the wrong number that is added when numbers are mistaken is often very near the right one. In normal cases, R. insists, forgetting is never due to the disagreeableness of the memory, but is always due either to fading of impression or to associative or reproductive inhibition.

The second part of the paper summarizes all of the current methods of measuring anterograde amnesia as opposed to the more recent

studies of retrograde amnesia with the results obtained. The summary of results is too long for complete statement. It is interesting to note that Ranschburg inclines to the view that patients in the excited stage of manic-depressive insanity have rather better capacity for learning and retention than in the normal condition. Characteristic changes are found in capacity for memory for each of the forms of alienation. It is interesting also that the perseverative tendency and associative retention may be differently affected and show different changes in different diseases. The congenitally feeble-minded, *e. g.*, show good perseveration with diminished associative recall, while the feeble-minded as a result of disease (paralysis, dementia præcox, etc.) have fair associative recall with defective impressibility and perseveration. Curves are given for the different memory activities of normal, feeble-minded, and delinquent children. Perhaps the most valuable part of the work is the summary of results of tests of different forms of disease that makes possible a differential diagnosis. The comprehensiveness of the study may be indicated by the fact that the bibliography contains 177 titles. The results are well arranged for reference in the body of the paper.

Of the shorter papers most are to be or have been published in other places and only abstracts are printed in the volume. Some few of those not to appear elsewhere may be briefly summarized. Spearman presents a modification of his method for the elimination of chance errors in the determination of the coefficient of correlation. It is based upon assumptions that give it a wider validity than the old method.

Exner gives a summary of different investigations which convince him that acquired forms of response may be inherited. Chauvin induced changes in the axolotl, ordinarily a water animal, by subjecting it to suitable temperatures, that made it change to a land animal as does the related amblystoma. This peculiarity was shown by the offspring. The strength of this argument has, I believe, already been turned by showing that the amphibious character of the axolotl is the usual one and the change may have been in no way related to the experiments in question. The other instances adduced are certain changes in the egg-laying habits of species of salamander, changes in the diet of caterpillars, and changes in the leaf rolling instinct of other caterpillars. Whatever one may think of the evidence, Exner's interpretation is of interest.

W. Köhler presents a suggestive note on the possibly discontinuous character of the tone qualities. His experiments grew out of the

tendency to refer a vowel quality to certain pitches. In two subjects he found that a note of about 265 VD was similar to *u*, 528 to *o*, 1054 to *a*. The suggestion is that these sounds have the same relation to the musical scale that the primary colors have to the spectrum. In answer to questions he asserted that there was a quality lower than *u* that corresponds to the *m* sound and probably others still lower that give critical points for sensational qualities as the vowel sounds do for the upper notes.

Stumpf and Hornbostle reported on the results obtained from a study of the phonographic records of primitive music that the psychological institute at Berlin has been collecting. Professor Stumpf discusses two conclusions that have already been reached. First the tendency to equal interval scales of five and seven tones, second the widespread use of fifth and fourth relations in part songs. In the Javanese five-tone scale the notes are related as $\sqrt[5]{2}$, the tones of the seven-note Siamese scale as $\sqrt[7]{2}$. These prove that the occidental scale is not the only relation that may be used in music. Stumpf rejects Wundt's suggestion that the scale has arisen from making the differences in the blocks of the zylophone or metallophone equal, for observation of the blocks shows that they have been hollowed out in the course of tuning. It is evident that the tuning is by the ear rather than by the eye, and that the scales represent a maximum of beauty to the natives. On the problem of the harmonic relations of simultaneous tones, the author reaches the result that the intervals of the octave, fourth, and fifth at first appeared by chance among a large number of other relations and were selected and retained because they were the more pleasant on account of the close approach to the unison effect that makes the words more easily appreciated. These are only preliminary to the many results that may be expected from a careful study of phonographic records.

V. Hornbostle in the next paper suggests other ways in which polyphonic music might originate. The antiphonic parts might easily overlap, first by chance, then be repeated intentionally when the effect was found to be pleasant, or the use of falsetto that is found in primitive music might, in singing with others, give a consonant effect with the voices of others and be repeated through its agreeableness. Possibly the interval of the major second that is found among the Admiralty Islanders might have been developed through the failure of some voices to sing in unison. He also remarks on the complexity of the rhythm and of the melodic structure of primitive music.

The interest taken in the instrumental side of the congress may be indicated by the fact that there were forty new pieces of apparatus exhibited in addition to twenty-five by Zimmermann, not all of which were new.

W. B. P.

SPECIAL REVIEWS

A MANUAL OF MENTAL MEASUREMENTS

Technique de psychologie expérimentale. Ed. Toulouse et H. Piéron. 2d ed. 2 vols. Paris: Doin, 1911. Pp. 303, 288.

This is a much enlarged edition of the volume published in 1904 by the same authors together with Vaschide. In its present form it constitutes a complete manual of tests for nearly all of the more usual experiments. After an introduction that describes laboratories (the Leland Stanford laboratory is taken as a type), apparatus in general and methods with sources of error, the work is divided into five parts: (1) measurements of simple sensory phenomena; (2) measurements of complex sensory phenomena (perception, attention and sensory affection); (3) measurements of phenomena of objectification (*Aussage* tests and reaction times); (4) measurements of intellectual phenomena (memory, association, and reasoning); and (5) measurements of individual differences. The only large fields that are not represented are the means of recording changes in circulation and respiration under the influence of stimuli. These are omitted explicitly because the technique has been developed for the most part in physiology and may be treated to the best advantage in that connection.

The discussion of the measurements of sensation is particularly full. Several new methods and instruments are recommended. One of these is the use of drops of water falling freely upon the skin for determining the temperature limen. The water is kept in a vessel provided with a thermometer, and an electric mixer and an automatic dropper, and the temperature may be changed at will by means of changing the current through a resistance in the vessel. A water dropper is also used to measure the limen for hearing. The drops are of constant size and fall upon an aluminum plate. The intensity is varied by changing the height. The relation of intensity of sound to height has been empirically determined by a method that is not d s bed. The relation obtained looks improbable, but without

checking the method no definite statement can be made. For sight the use of colored solutions is recommended everywhere, and empirical directions are given for preparing them. For determining limina of brightness, use is made of light diffused through an adjustable diaphram upon a ground glass plate. The siren is recommended for use in determining the difference limen for pitch, and for several other purposes.

In space perception the only unusual tests are those for the perception of solid figures on the skin that are taken over from the first edition. The tests of attention are restricted to measurement of the limen with signal and without and under distraction, marking letters or figures, reaction times and oscillations of attention. Less satisfactory is the list of tests on feeling which are restricted to arranging colors in order of affective value.

The instruments and methods for measurements of reaction times are described at considerable length. The chronoscope of D'Arsonval is recommended in this connection, on the ground that hundredths of a second are alone significant in mental measurements. The methods of making records of movements are also discussed at some length. This section constitutes, in fact, a good summary of method in that field for general reference.

The description of methods in memory has been very much changed since the first edition and now requires more than sixty pages. It is encyclopædic in its description of recent instruments and in the discussion of details of method, and makes several important original suggestions. Equally valuable although briefer is the chapter devoted to measurements of association.

Spearman is followed pretty closely in the discussion of the methods of determining correlations, and in this connection formulæ and tables are given for practical work.

The volume as a whole gives one a better impression of the full scope and possibility of the French experimental work than anything that has come under the notice of the reviewer. The descriptions are full, few short cuts are recommended, and the whole tone of the work is serious and the spirit that of painstaking striving toward the best experimental results attainable. It is comprehensive and even in all of its parts, and may be compared on equal terms with any similar work in any language.

It is interesting to note that the authors make definite selection from the different methods and either give only one or at least strongly recommend one when several are given. While the selection

is usually happy, in several cases it emphasizes the advantage of giving a number of methods as Whipple, for example, has done. Selection may be necessary for the tyro, but even he should be given a choice after some one method has been recommended for first trial.

W. B. P.

COLORBLINDNESS

Ein Fall von Gelbblau-Blindheit. S. Alrutz. Atti del V congresso internazionale di psicologia. Roma: Forzani, 1906. Pp. 217–218.

In view of the fact that there is a widespread interest in the facts of color vision and in color theories, it is peculiar that no notice has heretofore been taken in any of our psychological journals of the case of colorblindness which Professor Alrutz, of the University of Upsala, reported at the Rome Congress of Psychology. The importance of this general type of case is surely a sufficient excuse for a review at so late a date.

The subject, born in 1885, was a student at the University of Upsala. He was not aware of any change in his color sense, nor of any colorblindness in his family. He has had no difficulty in discriminating the red of berries from the green of leaves, but has had difficulty in distinguishing at a distance the blue hepatica from the green leaves, and in recognizing the yellow cross on the blue field in the Swedish flag.

In a test with the Holmgren wools, he confused the yellow-green, green-blue, and pure blue wools, with the green test skein; with the purple skein red and brown were confused, though he noted a difference in texture—the purple appeared uneven (fleckig); and with the red skein the purples were confused, though with the same reservations as to the homogeneity in the coloring of the reds.

An examination made with an arclight spectrum showed it to be perceived as unshortened. There was one color from the beginning of the spectrum in red, to a neutral zone, which began in yellow-green; this neutral zone he saw as grey 'sehr unbestimmt und ungesättigt'; beyond that, one color extended to the edge of the violet, where a second neutral zone was perceived; then red to the end of the spectrum.

An accurate determination with the Hoffman spectroscope '*à vision directe*' and with the sunlight gave the following results: The first color, which was called red, was most saturated at 655–650 mm. and most intense (lichtstarke) at 637 mm.; the saturation and intensity

decreased more and more toward yellow; the neutral zone lay between 570 and 562 mm., and had about the same intensity as the contiguous red on the left, and green on the right. The second color was called green, and was most intense at 507 mm., most saturated at 480 mm., and darkest in blue. A second neutral zone was found at the boundary between the blue and violet (467–464 mm.); it was still darker than the first. The third color, that of a newly advancing red, was darker, and not so homogeneous as the first red band, "aber doch etwas andersartig, als wenn etwas ausgefallen wäre, wie auf einem schwarzen Grunde."

A later determination with a Steinheils spectroscope, and a Nernst lamp, gave the following limits: Red, 790–770 mm. to 579–; neutral (white) 579–576; green 576–460; neutral (red and green) 460–450; 'red and black' 450 to beyond 400 mm.

The visual field is evidently not at all diminished. Red, yellow, and purple pass directly from white to red, and the green is constant. A dark blue passes from white, through red, into green.

An ophthalmological test showed that the colorblindness could not have resulted through any variation in the media of the eye.

Professor Alrutz is of the opinion that this case does not correspond to any case hitherto known.

FLORENCE RICHARDSON

DRAKE UNIVERSITY

REPORT OF THE COMMITTEE ON TESTS

Report of the Committee of the American Psychological Association on the Standardizing of Procedure in Experimental Tests. PSYCHOLOGICAL MONOGRAPHS. Vol. XIII., No. 1, 1910. Whole No. 53. Pp. 107.

In 1906 the American Psychological Association authorized the "creation of a permanent committee of the Association, to consist of five members, which shall act as a general control committee on the subject of measurements." It was recommended that this committee "undertake two general lines of work, organizing as many subcommittees as it shall see fit, and calling to its assistance such outside help as it may desire: first, the determination of a series of group and individual tests, with reference to practical application, and second, the determination of standard experiments of a more technical character." The present publication contains the reports of three members of the committee.

W. B. Pillsbury: Determination of the Intensity of Sound.—The report discusses the methods of the fall-phonometer, the controlled tuning-fork, the telephone, the audiometer, and several other means of measuring amplitude. The first form of stimulus is found to be incalculable and of uncertain quality. The second is less objectionable. But "taken all in all the telephone is probably the most satisfactory instrument to use for sound experiments in the psychological laboratory. It can be made to give a constant sound that can be varied at will in pitch, in intensity and duration. It can be applied to the measurements of intensity in all fields, is invaluable in experiments in rhythm when combined with an interrupting apparatus and has a number of uses in subsidiary experiments. In addition, the apparatus required is less expensive than many of the pieces sold as instruments for the measurement of audition and the parts can be used in many other ways." At the same time, "it should be remarked that all of the methods make certain assumptions in the calculation of absolute values that may be in error and if in error, would serve to account for some at least of the divergence in results. Obviously a highly desirable advance in acoustics is the removal of these sources of error and the development of methods more suited to direct measurements." The use of pure tones is strongly advised.

C. E. Seashore: The Measurement of Pitch Discrimination.—The selection and standardization of apparatus are carefully considered. Emphasis is laid upon the difficulty and the necessity of avoiding secondary criteria of pitch. The committee recommends the use of two standard-forks of 435 vd. and 10 increment-forks, giving the intervals above the standard of .5, 1, 2, 3, 5, 8, 12, 17, 23 and 30 vd. The forks are to be actuated mechanically and then placed for 1 sec. before a selective resonator to be heard and compared by the observer. Although the constant and variable errors arising from the apparatus, the experimenter and the observer appear to be quite as numerous and varied as those in Pillsbury's tests, they undoubtedly submit much more readily to calculation or to elimination. It is a noteworthy fact that Seashore and his collaborators should have found it feasible as well as profitable to reject elaborate apparatus and to reduce the material equipment to relatively simple means. The author has wisely sounded a note of warning against hasty inference regarding musical ability drawn from the test of pitch-discrimination. Psychology is still a long way from definite practical prescription in even so elementary a function as the judgment of tonal pitch.

James R. Angell: Determination of Mental Imagery.—Current

methods are rubricated under objective (memory, distraction, 'helps,' and Kraepelin's word-lists) and subjective-objective methods. The latter, which are very numerous, rely both upon introspection and upon the character of the test-material. Purely objective methods are thrown out as unreliable. The methods of the second class are of unequal value. In making his recommendations, Angell insists that the aims of tests for imagery are many and diverse, and that, in a given case, a determination must be fitted to its own particular purpose,—whether that purpose be classification under 'types,' the investigation of school-children, the range of imaginal forms, the preferred form, or the relation of the image to mental function. "No one of these aims can be realized by any single quickly executed test and one must squarely face the alternative of abandoning any pretense of accurate information concerning the imagery of a given individual, or else of undertaking a rather extended group of tests, whose outcome may be finally problematic, if the subject proves too deficient in introspective powers." Without denying the reality of 'types,' the author warns the experimenter that "the shifting and substituting of one form of imagery for another under slight changes of conditions, where no one could possibly have suspected the occurrence of such a transformation, speedily render the unprejudiced observer skeptical of all rigid divisions into types of the familiar kind." Two groups of tests are recommended: one short group "designed to furnish a *brief* survey of the imagery capacities of the subject," and another to offer "a more *intensive* and more *accurate* analysis." The first includes the questionary, the test on melodies, the 'description,' spelling, reasoning and writing tests. Save for the first, these "need not occupy over two hours." The longer series of tests recommended covers voluntary and spontaneous imagination, memory of the various sense-modalities, reasoning, and voluntary control, with specific instructions given under each heading. The author is sanguine that "such a group of tests as this intelligently applied, with the minor variations which may be required by the peculiarities of a given subject, will result not only in affording a highly satisfactory inventory of the individual's equipment of imagery, it will also convey a very significant impression of the actual distribution of his imagery and of the manner in which he uses it."

The reports show with what seriousness the committee has taken its instructions from the Association. The members have set about the reduction to a common denominator of dozens of half-related methods, at present scattered through the literature. The problems

covered by their reports are now in commission for profitable work by psychologists at large. In the reviewer's opinion, the scientific value of their labors rests less upon the formulation of 'tests' than upon the elucidation of a number of matters of pure psychology.

MADISON BENTLEY

CORNELL UNIVERSITY

JUDGMENTS OF DIFFERENCE AND MAGNITUDE

The Judgment of Difference with Special Reference to the Doctrine of the Threshold in the Case of Lifted Weights. WARNER BROWN. Berkeley: The University Press, 1910. Pp. 71.

The author made on a faithful and devoted observer 75,100 experiments on lifted weights and uses the results obtained as the basis for his discussions. The weights used were modified Cattell weights. The space error was not eliminated but kept constant. The size of the hand movement was not kept constant, but the time of the movement was regulated. A detailed table shows the percentages of correct judgments for all the stimuli used. It is seen that all the stimuli are misjudged at times and perceived correctly at other times, while the percentages of correct judgments increase with every increase of the amount of difference. There is no stimulus which is judged correctly in all the cases although some of the differences used are as large as 18 per cent. of the standard stimulus. If one tries to explain the wrong judgments on large differences by a temporary slip of attention, the question arises from which point on a mistake may be supposed to be due to such a slip. One then has to define this point in terms of the relative frequency of correct judgments and the author sees no reason why one point should be preferred to any other. There does not exist, therefore, a threshold of difference and whenever one was observed, it was due to some fault in the experimental arrangement.

The author requires that the judgments between which the observer has to choose should be mutually exclusive and that their significance should be independent of differences in the meaning which the observer might attach to them. The equality judgments do not fulfil these requirements and Brown believes that the observer should always be required to state which one of the stimuli seems to be greater. To test this view a series of experiments with very small differences was made and it was found that the cases where the impression of positive equality arises are extremely rare.

The most interesting part of the monograph is the description of an investigation whether the form in which a judgment is expressed has an influence on the judgment. Three series of experiments were made. In the first series the observer had to designate the heavier weight by pushing it forward with the hand. In the second the question had to be answered orally whether the second weight was heavier, and in the third the observer had to say whether the weights were equal or not. The standard weight was 100 gr. and the comparison weights ran in steps of 2 gr. from 82 to 92 gr., in steps of 1 gr. from 92 to 100 gr., and in steps of 2 gr. from 100 to 108 gr., making the number of comparison weights equal to 19. The number of judgments 'second weight heavier' was equal to 2,068 in the series with motor expression of the judgment and 1,952 in the oral series, the total number of experiments being in each case 3,800. From this it follows that the relative frequencies of the judgments in the two series are not the same, and that the judgment depends not only on the difference of the stimuli but also on the form in which the judgment is expressed. The author thinks that this is due to a bias or mental defect of the observer, an expression which is rather unfortunate.

Weber's Law is tested by observing the relative frequencies of correct judgments for the same percentual differences and different standard weights. If the law holds good, equal percentual differences should give equal relative frequencies of the correct judgments. It is found that this is not the case, since the graphs plotted for the different standard stimuli are almost parallel. The author concludes from this result that Weber's Law does not hold good for stimuli of the range under consideration.

Finally the influence of progressive practice and the variability of the judgments are investigated. The variations of the relative frequencies of correct judgments are very small in the series with large differences and there is almost no influence at all in the series with minimal differences. There exists, therefore, no influence of practice in these experiments although one might have expected to find one, since the observer was notified of every mistake. The variability of the judgments is investigated by the median values and the averages of the mean variations of the frequencies of the correct judgments. This part of the book is unsatisfactory on account of insufficient treatment of the data.

F. M. URBAN

UNIVERSITY OF PENNSYLVANIA

The Perceptual Basis for Judgments of Extent. H. L. HOLLINGWORTH. J. of Phil., Psychol., etc., **6**, 1909. Pp. 623–626.

The author uses results, which he has published in a previous treatise, for arguing against the view that the comparison of length of movement is made through the comparison of the duration of sensations arising from the movement. He favors the view that the perception of any one characteristic of a movement is just as primitive as that of any other and that the judgment of extent seems to be based on a system of signs which have been learned to mean extent directly.

F. M. URBAN

UNIVERSITY OF PENNSYLVANIA

The Central Tendency of Judgment. H. L. HOLLINGWORTH. J. of Phil., Psychol., etc., **7**, 1910. Pp. 461–469.

An indifference point is found in every series of magnitudes with which we work at approximately the midpoint of the scale. This phenomenon is explained by a law of central tendency, which makes our judgments on stimuli of a given range shift towards the median value of the series. This law is tested by experiments on the comparison of the size of squares, in which the influence of motor habit was excluded so that the errors were due to the judgment of visual magnitude alone. The central tendency of judgment reveals itself underneath the positive constant error which is present in experiments of this kind. The author finds that this central tendency can not be explained by any one of the factors usually introduced to explain the indifference point, but must be explained in its own terms.

F. M. URBAN

UNIVERSITY OF PENNSYLVANIA

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NOTES AND NEWS

PROFESSOR ZACCARIA TREVES, known for his studies on the psychology of work and fatigue, died April 28, at Milan, where he was director of the civic laboratory of experimental pedagogy.

PROFESSOR J. R. ANGELL, formerly dean of the senior colleges of the University of Chicago, has been appointed dean of the faculties of arts, literature and science, to succeed George E. Vincent, recently elected to the presidency of the University of Minnesota.

DR. J. F. SHEPARD, of the University of Michigan, has been advanced from an instructorship to an assistant professorship of psychology.

JOHN B. WATSON, professor of psychology at Johns Hopkins University, is to be one of the visiting instructors at the University of Chicago during the summer quarter.

THE present number of the BULLETIN, dealing especially with experimental psychology, has been prepared under the editorial care of Professor W. B. Pillsbury.

THE following items are taken from the press:

PROFESSOR G. M. WHIPPLE, of Cornell University, has been elected a member of the board of the Journal of Criminal Law and Criminology.

DR. BRUCE R. PAYNE, professor of educational psychology in the University of Virginia, has been appointed president of the George Peabody College for Teachers at Nashville.

PROFESSOR ANNA J. MCKEAG, of Wellesley College, will conduct courses in education and educational psychology at the Summer School for Teachers, Mt. Gretna, Pa.

PUBLISHER'S NOTE.—The *Psychological Review Company* has been incorporated by the editors of the Review Publications. Howard C. Warren is President of the new company, James R. Angell, Vice-President, and John B. Watson, Treasurer. The Board of Directors include the officers and Arthur H. Pierce and H. C. McComas. The new company has purchased the entire business of the Review Publishing Company and will hereafter publish the PSYCHOLOGICAL REVIEW, BULLETIN, MONOGRAPHS, and INDEX.

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THE

PSYCHOLOGICAL BULLETIN

GENERAL REVIEWS AND SUMMARIES

VISUAL SPACE

BY PROFESSOR G. M. STRATTON

University of California

In the contributions of Watt (25), Karpinska (13), and of Jastrow (12) one finds a certain tendency to lessen the rôle that has long been assigned by many to the strictly binocular factors, namely of convergence and bi-retinal disparity. Watt announces that he has obtained most vivid plastic effects by presenting to the single eye in rapid alternation the usual disparate stereoscopic views. This he best accomplishes by a double projecting lantern in connection with a rotary disk that cuts off one view as it exposes the other, at a rate of about 0.2 sec. for each single phase. He is also convinced by actual trial that depth-effects truly binocular in quality can be had for the single eye by mirroring in rapid alternation disparate views of actual objects; and like effects, too, when there is any rapid translation of the eye, or of objects with reference to the eye. His theory is, that the binocular mechanism is but a special device for obtaining, under conditions of rest, the results which were previously provided for by rapid translation of the eyes, and which are still so obtained for the single eye when all is suitably arranged; in sum, that there is no peculiar or novel psychic quality in binocular vision. Karpinska (13) using both the stereoscope and the verant with exposures of minimal duration finds other ground for regarding bi-retinal disparity and convergence as of lessened importance. It has of course long been known that with instantaneous illumination of the stereoscopic card, and consequently an exclusion of actual change of convergence, there was still a true binocular depth. Karpinska's observations bring out the *noticeably gradual on-coming* of this depth-

effect even with instantaneous exposure, and the probability that some additional and inner process coöperates with and reinforces the prime effect of the disparity. Furthermore, this inner process can produce the full plastic effect without any bi-retinal disparity; and on the other hand the disparity is no guarantee for the on-coming of the plastic effect. For by irregular use, now of identical views, and now of disparate views, it is found that identical pictures in the stereoscope often give a plastic effect, and disparate views give a 'flat' effect; while with the verant the instantaneous view is normally plastic. Jastrow (12), by experiments in which care was taken to exclude the suggestive effect of *knowing independently* the general limits within which the selected depths would range, found that, when dependent on mere convergence, the judgment was subject to much the same aberrations as are found when it depends on mere accommodation. Points of light from five to twenty-five feet away "may be judged as close as five feet, and as far away as one hundred yards." All in all, then, the estimate of the peculiarly binocular factors would by these studies appear to be lowered relatively to the factors common to both binocular and monocular vision.

Von Kries (14), from the experiments of several workers under his direction, finds that in the excentric visual field, as we have long known with regard to the foveal region, our power to discriminate differences of depth is far more precise than is our power either to perceive the doubling of images or to control the convergence of the eyes in such a way as to bring two images to union. The threshold value for depth-discrimination, as we might expect, falls off very rapidly, the farther out we go from the fovea; the indication from these experiments is, that the decline for depth is more rapid than for visual acuity. Von Kries will not as yet, however, affirm this, but prefers to draw special attention to the great precision, which the experiments make evident, in the combined use of the two eyes even for points at a considerable angle from the center of vision.

While von Kries's results thus indicate a close functional connection between excentric points in the different retinal fields, Berger (3) would confine retinal correspondence to a region within about 10° of the fovea. Beyond this limit he fails to find, by means of the stereoscope, either diplopy, or retinal rivalry, or binocular color-contrast, or lustre. In these results, if confirmed, there is nothing markedly at variance with von Kries's report, the region observed by whose students slightly exceeded in but a single instance the limit within which Berger would deny the existence of retinal correspondence. On the

other hand, the same investigator finds (1, 2) that toward the center of vision there is often an irresistible impulsion to bring to corresponding positions and consequently to fusion, images that in the first instance lie on points far from corresponding. Thus with the stereoscope he finds that images disparate, occasionally, as much as 1.5° apart vertically, and 2.5° horizontally, will, in spite of contrary suggestion and in the face of the usual coördination of the eyes, be brought to fusion by a hesitant turning of one eye's line of regard up or down from the common plane. The author perhaps takes no sufficient pains to make clear that the fusion does not actually take place while the images are still upon points far from correspondence. The fact is, merely, that the impulse to upset the usual coördination in favor of a corresponding retinal position is aroused even from points of considerable disparity.

Von Liebermann's contribution (16) has to do with the comparison of central with excentric depth; the determination of a surface all of whose points appear to lie in a frontal plane passing through the fixation point,—a surface composed of so-called 'abathic' points.[1] He finds that this surface may be unsymmetrical, and does not maintain a constant form as the distance of the fixation point changes, but decreases in curvature with increased distance of fixation. The 'abathic' surface, therefore, is not fixed exactly by the conditions that determine the physiological horopter. In closing the account of matters connected especially with binocular vision, mention may be made of two proposals close upon the mechanical side of stereoscopy. Wiener (26) discusses the possibility of obtaining by selective glasses and white lights of different chromatic composition the effect which is now had by the use of red and green light, respectively, for the images of the two eyes. Estanave (5) describes a device by which, from a single plate, different scenes or different aspects of one scene may be observed from different points of view. In principle it is not unlike what we long have had in street advertisement, where with change of place the passenger reads, from the same surface apparently, now one phrase and now another.

With regard to monocular space, Heinrich (9) reports briefly an investigation analogous to that of von Liebermann, already referred to, in the binocular field. For certain selected depths (35, 45, 60, 80 cm.) the attempt was made to find the shape of a surface all of whose points appeared to be at a distance from the eye equal to that of the fixation point. Lines of astonishingly great curvature, concave

[1] *Isobathic* would seem to the present reviewer a more fitting expression.

with reference to the eye, were found as the locus of such isobathic points, and are graphically presented. The curve, as von Liebermann found in his study, may be unsymmetrical. And if by the thickness of the 'line' we represent the width of the zone of apparent equality, the line increases in thickness with increase of the angle of excentricity. Furthermore, even for a single fixation point, the locus of isobathic points is declared to be in some cases not a single line but a series of more or less similar curves, one behind another. Kuntz, who was the observer in these experiments reported by Heinrich, himself describes (15) a further phenomenon in connection with isobathic points. Two such points, it seems, cease to be isobathic after 20–60 seconds' observation. There then occur fluctuations between apparent equality and apparent inequality, which Kuntz has timed. The periods are variable, in his tables, between 0.78 and 11.22 seconds, as extremes. The author senses some connection between these alternations and the familiar fluctuations of perspective in ambiguous drawings.

Visual direction is the topic of a group of studies. Haberlandt's experiments (8) were concerned with the error in the reproduction of visual changes of direction, according as the changes were noted (*a*) with eye-movements, (*b*) with head-movements, and (*c*) with head-movements and eye-movements combined. The results with his two observers were conflicting: the one, emmetropic, made his smallest average errors with eye-and-head-movements, while pure eye-movements gave largest errors; the errors of the other observer, myopic, bore a relation exactly the reverse. Delay in the subject's indication of the direction, a change to 60 seconds, instead of 2 seconds, of time (*i. e.*, lengthened memory of the offered direction) produced no clear and regular result. Various complications, by which, after the stimulus-direction had been given, the observer indicated it only after certain ocular excursions had intervened, very naturally increase the error of the indications. Hofmann's experiments (11) start from the observation by Aubert, that with rotation of the head to left or right upon a horizontal axis, the apparent visual vertical or horizontal suffers a dislocation; and also from the observation of Hofmann and Bielschowsky, that with upright head a rotation of the background likewise dislocated the apparent vertical. The present experiments give the amounts of such dislocation for various angles of rotation of head and of background. In general the dislocation from these two factors if they are in the same direction is in the same 'sense,' and tends to summate; with opposite directions of rotation

of head and background the tendency is for each to neutralize the effect of the other. Interesting asymmetries appeared; with Hofmann himself, for instance, inclination of the head to the right produces a greater dislocation of the apparent vertical than does a like inclination to the left. Nor need the asymmetry of one observer be the same as that of another. The author believes the dislocation, when lines are used for background, to be independent of Zöllner's illusion.

A controversy with regard to visual direction has arisen upon Witasek's observation that when the two eyes look successively at the same actual point (say, by screening now the one eye and now the other) this point does not appear to lie in the same direction in the successive observations. Hillebrand (10) attributes to heterophoria this apparent shift, and holds that, in spite of appearances to the contrary, the visual direction of the point is the same in the successive observations, because in them all it bears an unchanging relation to the total system. Witasek, on the other hand (27), maintains that these shifts are proof that the directions of monocular space differ from those of binocular space, and that the Law of Identical Directions needs amendment. He scouts the attempt to explain the facts by heterophoria when clinical tests give no evidence of its presence; he even suggests that heterophoria has become a mild obsession with Hillebrand. Von Sterneck's contribution to the discussion (24) is, that the apparent shift is found to change in amount and even in direction with different distances of fixation; he rejects both Witasek's and Hillebrand's explanation, and offers instead what he considers a more psychological theory, that we have here an illusion due to the fact that in monocular vision we are employing sight in an abnormal way, for it is normally binocular. Hillebrand curtly replies to Sterneck that he shows ignorance of dynamic heterophoria. The controversy shows scientific acumen, mingled with verbal disagreement, and all suffused with animal heat.

A somewhat kindred problem is touched by Ovio in two of his papers (18, 19), where he contends that the visual angle should be expressed with reference to the 'principal' point rather than to the nodal point, siding thus with Gullstrand and (to all intents and purposes) Helmholtz, as against Listing and Donders. His chief argument is that by change of accommodation we may shift the nodal point without affecting the apparent size of the object. The visual angle, consequently, which varies directly with the size of the image, has its apex not at the nodal point but at the principal

point of the eye. In a like spirit he holds that the visual projection of points should be described, not as along the nodal line universally, but in a direction occupied by the nodal line usually.

Visual acuity for the excentric portions of the retina has been carefully studied by Poschoga (**23**). He finds, contrary to what is true of dermal space, that the threshold for spatial twoness is in general not so low when the stimuli are applied successively as when applied simultaneously. By each method of application he investigated sixteen different excentric regions: eight at a distance of $22\frac{1}{2}°$ from the center, eight at a distance of 45°, lying on the four chief and the four diagonal meridians of the eye. The advantage of the simultaneous over the successive application of stimuli the author believes to be explained at least in part by the fact that with successive application there was a strong impulse to eye-movements; while with the rival method the judgment depended almost entirely on the purely retinal report. In each of these sixteen different regions, and by each method, Poschoga applied his two stimuli, now the one above the other, now above and to the right of the other, now directly to the right, and so on, boxing the compass through the our principal and the four diagonal directions. The threshold values for all these different combinations when plotted gives an interesting system of 'threshold surfaces.' These surfaces are more nearly circular for simultaneous stimuli, while for successive stimuli they are elongated in the direction of the fovea. For this difference of shape an explanation is offered, in which the well-known tendency of memory to distort the unusual into accord with the usual, is given a prominent place. Poschoga finds in his results abundant ground for rejecting the idea that the spatial threshold in excentric vision is fixed by the purely anatomical conditions of the retina.

Ovio (**20**) makes detailed calculations and drawings to show the changes which various inclinations of a printed page produce in the visual angle of the letters and consequently in the practical acuity; and he reports the result of actual tests. His more important finds are that the reduction of acuity by such inclinations is not the same for isolated letters and for passages of text; nor is it the same for 'caps' and or 'lower case'; nor is it the same for an inclination of the top of a book away from the reader or toward the reader as for a like inclination right-forward or back. With a book of ordinary size laid flat on the table before the reader, perspective distortion can reduce the virtual acuity as much as one half. Perspective considerations favor large letters, short lines, proper spacing of letters and lines, and small format.

Our knowledge of the perception of movement is furthered by certain sections of Marbe's work (17). He finds that when a row of separated points are made to flash in regular succession the appearance of motion thereby induced passes through several distinct stages as the rate of succession is increased: from a jerky motion spatially interrupted, through a jerky motion spatially continuous, to a uniform and continuous motion, and thence to a final stage where the rate is too high to give any apparent motion. The rates of succession that give these different appearances were determined, both for freely-moving and for fixated eyes, the latter condition requiring in general higher rates. I shall make no attempt to give his application of these facts, and of others connected with flicker and with Talbot's law, to kinetoscopic projection.

The problem connected with the apparent difference of size when sun or moon are near the horizon and when either is in the zenith seems almost as enduring as these heavenly bodies themselves. Filehne (6) opposes Zoth's theory that this difference is due merely to the direction of the line of sight, and would have it that, while the direction of this line is of influence, its influence is quite insufficient to account for the full illusion. Filehne has observed the rising moon with elevated line of sight, and the moon aloft with eyes in the primary position, and all without obtaining the results which Zoth's theory would require. When he observes objects through a suitable black tube, however, he finds a marked reduction in the apparent size of objects viewed with level sight. This effect of the tube is due, he believes, to its exclusion of objects in the lower stretches of the field of view, in comparison with which we have been taught by experience to overestimate the size of objects more near the level of the horizon where the greater distances are found. The rising moon viewed through such a tube is reduced in apparent size, he finds, by more than a half. In a companion paper (7) he addresses himself to Zoth's smoked-glass evidence for his *Blickrichtung* theory. Filehne attributes the persistence of the illusion when sun or moon are viewed through smoked glass, not to the mere difference of direction of the line of sight (for with smoked glass, too, he finds the long-risen moon looking small even with eyes in the primary position, and the just-risen moon looking large when viewed with up-turned eyes) but to the inevitable difference of projection to which experience has trained us. We have grown familiar with horizon-objects at great distances, and accordingly see the horizon-moon as farther and therefore larger. Objects seen in directions well above the horizon are not mentally forced to such extreme distances. The apparent shape of the

heavens is, for Filhene, just the sum of the various limiting projections to which long experience has schooled us.

Questions that lead where psychology passes over into philosophy are kept fresh by certain of the contributors. In the study of hemianopsia, Best (4) is interested in the direction of the line that divides the seeing from the blind portion of the field, especially in those cases where this line cuts through the central area without skirting it. He finds, in a case he has studied with especial care, that the dividing line runs not more than 8.5′, angular measure, from the fixation-point, and in a direction within 7′ of the true vertical. Such an 'anatomical vertical,' when allowance is made for the peculiar difficulties and errors of measurement here, is close to the apparent vertical in the normal eye; and taken in connection with certain facts which, he believes, indicate that the blind half is still of influence both upon contrast and upon visual direction, Best holds them all as favoring a nativistic view of visual space.

On the other hand, Nativism in the Kantian sense is combated by Pitkin (22) after describing the look of things at the limits of the visual field and on the borders of the blind spot, where he sees present in the flesh spatiality without spatial form. Moreover for many modern writers the Kantian view, he holds, is paradoxically conjoined with a view of evolution whose very setting and make-up is of space and time. Space is thus regarded as a mere form in our experience and at the same time an essential feature in reality itself. Pitkin argues for realism in which extension is not just a 'sign' or 'mental state' but a true cosmic quality.

A systematic treatment in due order, of the more familiar problems of visual space is given in a volume by Witasek (28), the first of a series to be written by many hands entitled *Die Psychologie in Einzeldarstellungen*. The author has attempted in his work to subordinate physiology to psychology. And yet it is a book to which one will hardly go for those full and precise details of psychological discovery which we have come to expect in a treatise on this topic. It leans, by preference, to discussion and the development of a system in which perhaps the most prominent general feature is the attempt to develop the idea of a space-sensation, connected on the one side (like any other sensation) with its 'stimulus,' namely actual space; and on the other side, with the various mental structures in which the raw 'sensation' of space appears psychically modified and elaborated. A report of the more recent literature should not close without mention of the very full review by Pfeifer (21) of the publications of the year 1908, abstracted, classified, and indexed.

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AUDITORY SPACE

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Recent contributions on the localization of sound deal mainly with the factors on which the perception of direction and distance depends.

Dunlap (1) made a comparison of monaural and binaural localization. The stimulus was given at forty-eight different positions on the sphere described by the arm of Pillsbury's sound perimeter. The observers indicated the estimated locations either by pointing or by marking the position on a chart. When only one ear was used the other ear was closed with cotton and paste. The results are rather indefinite. There seems to be very little relation between the estimated and the actual location of the sounds. Each observer showed preferences for locating all sounds in certain regions, but these preferences varied from time to time. The principal difference between monaural and binaural localization seemed to be that in the former case the sounds were displaced toward the ear in use.

Intensity as a factor in determining distance was the problem of Miss Gamble's (3) investigation. Two series of experiments were made. In the first series it was shown that the perception of liminal differences in distance roughly conforms to Weber's law. In the second series two stimuli were given which were either of the same intensity but at different distances, or of different intensities but at the same distance. The observers, who did not know how the stimuli were manipulated, stated on the basis of their observations what the differences seemed to be.

A large number of judgments were obtained from numerous observers with the positive result that "intensity is the main criterion in estimating the distance of sounds." 'Nearer' and 'louder,' and 'farther' and 'softer' are practically interchangeable characteristics.

The experiment made by Starch and Crawford (4) relates to the measurement of the distance threshold in different directions. Thirteen directions in the right semicircle, on a level with the ears, were tested to determine whether the least perceptible difference in distance of a uniform stimulus was constant in all directions. The measurements on eight observers consistently indicated that the distance threshold is uniform in all directions tested and that it is approximately one sixth of the absolute distance from the head. The tests

also corroborate Miss Gamble's findings that the perception of distance depends mainly, if not entirely, upon intensity.

The most extensive experiments are those of Ferree and Collins (2). Their object was to demonstrate three arguments for the binaural ratio of intensity. (*a*) Observers naturally more sensitive in one ear should show a constant tendency of displacement toward that ear. (*b*) Observers in whom a difference in acuity is produced artificially should manifest the same tendency. (*c*) A natural tendency toward right or left displacement should be subject to correction by making the proper change in the ratio of acuity.

The experiments were conducted by means of Titchener's sound perimeter. The stimulus was a 20,000 vibration Galton whistle tone. The acuity of the ears was determined by the watch test. Ten observers were employed, but the records of only three are given.

One observer whose ears were equally sensitive showed a slight displacement of 2.1 degrees toward the right side. Another observer in whom the ratio of natural sensitivity of the right to the left ear was as 4 to 1 showed an average displacement of 20.3 degrees toward the stronger ear. The third observer in whom the ratio of sensitivity of the right to the left ear was as 3 to 1 showed a displacement of 10.7 degrees toward the stronger ear. Differences in acuity produced artificially yielded similar results.

Individual differences indicated by Dunlap were also found in these tests, but they seemed to be permanent in the same individuals.

A valuable part of this article is the historical survey of the field since the appearance of Pierce's work in 1901.

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TACTUAL AND KINAESTHETIC SPACE

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In his first paper (1) Basler investigates the problem,—how great must a movement of a stimulating point along the skin be in order, first, that the movement as such and, second, that its direction be

tactually just perceived? The apparatus consisted of a pencil of hard rubber tapering to a point about 1 mm. in diameter; this was inserted vertically near the fulcrum of a one-armed lever, moving horizontally, the free end of which moved along a scale. The magnification on this scale of the movement of the point was about 50. The part of the skin to be investigated was placed on a support over and in contact with the point which was then given various extents of movement along the skin. On the cushion of the forefinger movements of from .02 to .03 mm. were just perceived, with a pressure of from 3g. to 10g. The sensitivity of the forefinger is as good, concludes the author, as that of the retina at a distance from a moving object of 30 cm., at which distance a movement of .02 mm. is just perceptible.[1] The results were unchanged when, instead of a point, a straight edge, or a disc, 4 mm. in diameter, with a rough surface, was used on the skin. On the forearm, about 4 cm. (proximally) from the hump of the scaphoid bone, the threshold was from .5 to 1 mm. These results are smaller than those of Hall and Donaldson,[2] doubtless because the movement of the point was oscillatory (about 10 back and forth movements for each observation) while Hall and Donaldson made for each only one movement.

The perception of direction was not so accurate, the threshold being about .05 mm. for the finger—a smaller sensitivity than for the eye.

Since the thresholds obtained in these experiments were much smaller than those obtained by v. Frey for successive touch, the explanation doubtless is that the moving point occasions slight displacements of the skin which in turn stimulate the sensory touch endings.

In the second paper (2) Basler studies the minimal speed of a distinguishable movement on a touch area. The finger rested on a fixed cork surface with the finger cushion over a hole 1 cm. in diameter; from below an ebonite point (1 mm. across) just touched the finger. This point was fixed vertically to a horizontal rod movable at any desired speed by a clockwork. The end of the rod was provided with a marker recording on a kymograph. A Jaquet chronograph was used for the time line. After the clockwork had been started (the

[1] Basler, A. Über das Sehen von Bewegungen. II. Mitt. Die Wahrnehmung kleinster Bewegungen bei Ausschluss aller Vergleichsgegenstände. *Arch. f. d. ges. Physiol.*, 1908, **124**, 313–325.

[2] Hall, G. St. and Donaldson, H. Motor sensations on the skin. *Mind*, 1885, **10**, 557–563.

kymograph having already been set going) the subject was to state whether he felt a movement. The movement of the point was transverse to the finger axis. After practice the threshold was (for the left forefinger) .05 or .06 mm. At the threshold a movement would often not be felt for several seconds. For the ball of the thumb the threshold was about double, a line of hard rubber, 4 mm. long and moved at right angles to its length, being used, however, instead of a point. For the forefinger the *direction* of movement was always distinguishable at a speed of .15 mm. per second. A subsidiary experiment concerned the rapidity with which voluntary movements for the purpose of tactual estimations (*tastende Bewegungen*) of objects are made. A wire arch was placed over the dorsal side of the forefinger and attached to a movable horizontal rod which recorded on a drum. The volar side of the finger tip could thus be moved back and forth transversely over wires of various diameters wound closely about pieces of cardboard. The subject was simply asked to feel the presented surface and decide whether it was smooth or rough. The average normal speed of such movements, in different subjects, was 30 to 40 mm. per second, the slowest being 17 mm. and the fastest 100 mm.

Treves (3) reports a series of experiments on blind children of different ages testing the accuracy with which they could reproduce positions of the limbs, execute movements of given directions and extents, draw lines and figures (verticals, horizontals, squares, right angles), etc. They were designed chiefly to suggest practical exercises in educating the blind.

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SPACE ILLUSIONS

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Foucalt (10) contributes a critical digest of the literature on the paradoxical illusion and the two point threshold. He is able to suppress or produce the illusion at will mainly by a manipulation of

the order of the series. Suppression of the illusion is a necessary condition for a valid determination of Weber's threshold. Introspective data are emphasized and the phenomenon is explained in terms of tactile imagery. Hémon (11) presents an interesting experimental study of the experience of feeling parts of the body lost by amputation. Some never experience the illusion, some continually experience the lost member and others feel the phantom member only when the stump is stimulated. In this case the sensation is localized at the extremity of the phantom member, and this latter may be made to disappear by a stimulus applied to the opposite limb. A theory as to the spatial localization of cutaneous sensitivity is broached. In discussing the illusions of amputation and Stratton's experiments upon inverted vision, Pick (17) concludes that the basis of reality judgments in hallucinations is to be found in the lack of conflict between the hallucination and the whole perceptual field. Ponzo's article (18) (printed in Italian) is followed by an excellent digest in German by Kiesow (12). It demonstrates by a variety of tests that a contact on any displaced part of the body tends to be localized normally. The Aristotelian illusion is regarded as a special case of the principle. One may obtain also a single touch with two disparate contacts. Ewald (8) gives an apparatus by which this reversal of the Aristotelian illusion may easily be obtained. The strength of the illusion varied with the method of localization used. Miss Cook (6) presents an excellent study of the illusion of filled and empty tactile space. With two groups of subjects, the same area was stimulated successively and the filled magnitude was underestimated. Simultaneous stimulation of adjacent areas gave an overestimation of filled space. The illusion was practically nil when adjacent areas were stimulated successively. The facts are interpreted in the light of introspective data. The illusion can not be correlated with the functional efficiency of visual imagery; it depends mainly upon the distribution and stress of attention and the mode of apprehension of the magnitudes compared.[1]

In his experiments upon the pendular illusion, Ford (9) accepts the after-image and limen hypotheses, though he gives less importance to the latter factor than did Carr. His tests show the strong influence of a third factor, *viz.*, the direction of attention whose effects are interpreted in terms of the law of prior entry. Dodge (7) replies critically to the work of Ford and Carr. He contends that their experiments do not constitute a critical test of his conception because

[1] See Special Review on p. 256.

(1) after-images were not present in his experiments, (2) the presence of gratings and stationary lights offer a fixed point of reference, and (3) an essential condition of his experiment was the presence of true pursuit movements and Carr can not guarantee that this condition was fulfilled, while attention to the peripheral light will destroy such movements. The fact discovered by Ford that the illusion occurs when the peripheral light is stationary is held by Dodge to prove the validity of his conception of the functional inefficiency of eye movement in the perception of motion. Carr (5) adds some new facts to the autokinetic phenomenon. Two types of illusion were noted, in one of which eye movement was demonstrated. Fatigue due to previous constrained eye position has a pronounced effect with some subjects. Strain of the ocular and associated muscles and its resultant fatigue were effective. By the after-image method, no correlation was detected between the illusion and involuntary bulbular twitchings. Stratton (19) finds that a diasclerotic light may sometimes arouse a localization on both sides of the visual field. The light stimulates both sides of the retina and the normal law of projection obtains.

In the field of the geometrical-optical illusions, Lewis (13) concludes that the illusion of contrast is the limiting case of the Müller-Lyer figure and that both are to be explained in the same terms. The maximal effect obtains with variation of length of arms, but not with angular variation, although the position of the maximal effect as the arms are varied in length is a function of the size of the angle. The results confirm the factual statements of Wundt and Heymans. The facts are explained in terms of the opposing principles of confluxion and contrast. The technique and procedure of the experiment are to be commended. Smith and Milne (14) noted that the presence of the vertical marginal line on common ruled paper is influential in the attempt to bisect one of the ruled lines. The effect varies with individuals and the distance of the vertical line from the edge of the paper. Confluence is the principle of explanation supported. Botti's article (3) is mainly an expository summary of results published elsewhere, a fact which renders any critical evaluation difficult. By a number of figures, he shows that in perceiving the direction or extension of a line, one tends to judge it as being perpendicular to a series of parallel crossing lines. This effect can be negated by divers devices. The principle is applied to the figures of Poggendorf and Zöllner and to the angular illusion. The results seem interesting and important.

Becher (1) and de Boer (2) both criticize the Wundtian principles as the sole explanation of reversible perspective. The complexity of the phenomenon is noted and the influence of apperceptive tendencies is emphasized. Besides general observations and critical discussion Becher offers the results of tachistoscopic tests. De Boer noted the first spatial form apprehended in a short fixation and found that relief occurs more frequently than depth, a fact which is consonant with common experience. Habitual motives due to past experience are regarded as the main determining factor. Thierfelder (20) notes a reversible illusion when regarding a rotating wheel from one side. The form reversal is accompanied by a reversal of the apparent direction of rotation. Burmester (4) calls attention to the fact that the illusion is not new and gives historical references.

Müller (16) discusses the apparent brightening of objects when regarded through a telescope at dusk. The objective intensity is decreased and as a consequence of Weber's law the brightness difference between the object of regard and the darker environment is increased. The object is perceived more distinctly and this distinctness is interpreted as an increase of intensity. Müller (15) is concerned mainly with a criticism of an article by Pozděna who assumed that the moon is perceived as projected on the plane of the sky and as a consequence the apparent size of the moon is directly proportional to the apparent distance of the sky. The assumption is criticized on the basis of theory and fact. Several instances are given showing that the two phenomena are independent variables.

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RHYTHM AND TIME

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The most important recent contributions to the psychology of rhythm are by Woodrow (**5**, **6**). In his first paper he develops a method of converting 'iambic' rhythm-groups into 'trochaic' and *vice versa*, and 'dactylic' into 'anapestic' and *vice versa*, by increasing the internal interval preceding (or following) the accented stimulus, at the expense of the external interval. The amount of increase in the internal interval just necessary to destroy the original grouping, Woodrow names the 'indifference point,' and considers it to be the measure of the 'amount of rhythm' in the original series.

In auditory series with accent (higher intensity) on each second sound, and 'measures' of 1.5 sec., if the external and internal interval were equal the subjects invariably formed two-groups with the more intense sound leading. With every third element more intense than the other two, with 'measures' of 2 sec., the subjects formed three-groups with the stronger sound leading, when the intervals were all equal. In both cases the 'amount of rhythm,' as measured by the 'indifference point,' increased with the relative intensity of the ac-

cented sound. Similar results were obtained with series of electric shocks. In auditory series in which each second or third sound was of greater duration than the others, the subjects uniformly commenced the groups with the longer sound, if the intervals were all equal. In this case, the author claims, the 'amount of rhythm' decreased as the duration of the long sound increased; the reverse however is true if the reckoning is by duration-units—from beginning of one sound to beginning of the next sound—instead of by empty intervals. In accented auditory series in which the rate was varied between 0.5 and 7.0 sec. to the 'measure', the 'amount of rhythm' was found to increase with the rate. Woodrow thinks that the rhythmic grouping is altogether a matter of temporal relation, and that accent (intensity) determines grouping through the overestimation of the interval preceding the accent.

In his second paper (**6**) Woodrow takes up the effect of pitch-differences on the auditory rhythm. With 'measures' of 1.5 sec. he found that some of the subjects commenced the groups with the higher pitched note, and some with the lower. Increasing the pitch differences produced neither uniform nor significantly large changes in the 'amount of rhythm.'

The most serious criticism of Woodrow's general method is that it may render the subject especially open to suggestion of grouping in accordance with the *terminus ad quem*. His manner of transforming data however—rearranging the series of judgments to bring into them a consistency the subject did not achieve—is astonishing, to say the least.

Two studies by Wallin (**3, 4**) are worthy of notice because of the importance of the problems upon which they touch. The first paper gives results of work in which the weak element of each third measure of 'iambic' or 'trochaic' auditory rhythm was displaced (delayed) by the small step procedure. The subject was required to classify the rhythms in five categories, from 'excellent' to 'disrupted'; to make a 'time judgment' on the displacement; and to give introspection on points specified in a list of nine items followed by 'etc.' Only one series was taken on each subject. Two rates of stimulation were used: 0.57 sec. and 1.075 sec. from beginning to beginning of the sounds, which were controlled by a form of the Mayer apparatus rotated by a kymograph. Interpretation of the tabulated results is exceedingly difficult, but the reviewer infers that the amount of displacement necessaryto take the rhythm out of the 'excellent' class was from 0 per cent. to 33 per cent., according to

subject. How much displacement was necessary to 'disrupt' the rhythm, cannot be found out from the tables. The results of the 'time judgments' are given only in averages for all subjects together; the averages for the just noticeable displacements range from 4.6 per cent. to 7.3 per cent.

In his second paper Wallin (4) gives the results of an attempt to determine the preferred rate of auditory stimulation, using a metronome permitting 31 rates from approximately 40 to 208 strokes per second, and proceeding by a short method of paired comparison. The final preferences of the twenty subjects (one series) range from 0.305 sec. to 1.37 sec. stroke-period. Although eighteen of the subjects grouped the sounds rhythmically, no serious attempt was made to find the relations between groupings and rates—the main point in investigations of this type. The remainder of the paper describes observations on the rhythmic actions by which patrons of the theater gallery accompany the music. It is unfortunate that the author should have allowed either of these papers to go into print in the shape in which it is.

Arps and Klemm (1) sought to determine the position in the interval between auditory stimulations occurring at a uniform rate, at which the visual difference threshold is lowest. The positions used were 0.06, 0.44, 0.78, 1.50 and 1.81 sec. after the auditory stimulus (hammer stroke), which occurred at the rate of 1.96 sec. per stroke. The visual stimulus was produced by three incandescent electric lamps, which were increased in brightness by the momentary (82 sigma) lowering of the external resistance. All stimuli were controlled by the Wirth pendulum. The data obtained show the lowest threshold just before the stroke. The authors sought also to determine the threshold for displacement of one element in a series of eighteen strokes, each third stroke four times as loud as the others, forming six groups ('dactylic') with 0.65 sec. nominal stroke-period. The data show the lowest threshold for the accented stroke, and the highest for the second weak stroke in the group.

Careful examination of the description of these experiments raises the presumption that the experimenters did not take into account the error resulting from the time required for the hammer to fall after the breaking of the current through the supporting magnet nor of the still larger error due to the delay in the heating up of the lamp filament. The current-increment which occurred 150 sigma before the breaking of the hammer-circuit probably produced a light-increment considerably later; possibly after the hammer-stroke.

The 'dactylic' rhythm probably involved an interval of 0.65 sec. between the two strokes, a longer interval preceding, and a shorter following, since the intensity changes here were obtained by changing the speed of the hammer-fall. This presumption is strengthened by the fact that the subjects 'over-estimated' the interval after the accent and 'underestimated' the interval preceding. Many investigations on (or with) auditory rhythmic series have been spoiled by neglect of the hammer-error; as for example, Salow's work on the feeling-character of rhythmic sound groups (*Psychol. Stud.*, **4**, 7–8).

Geigel (**2**) determined the minimal perceptible time-interval between two sounds of nearly the same character. His apparatus was a small cylinder of wood sliding on a rod projecting from a wooden disc, the whole dropped from various elevations onto a sheet of blotting paper. The spindle struck before the rod, the interval between the two strokes being smaller as the height of fall was greater. Computing the intervals from the height of fall and the length the cylinder slid on the rod, 10.66 sigma gave a *doppel* tone, and 9.64 to 5.93 sigma gave a *gespaltene* tone. At the next step, 5.74 sigma, no *Spaltung* of the sound was perceptible. Extreme allowance for the probable effect of air resistance gives 8.10 sigma and 7.02 sigma for the last two measurements, hence Geigel concludes that an interval less than 7 sigma between the strokes was perceptible. With weaker sounds, still smaller intervals sufficed.

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MEMORY, IMAGINATION, LEARNING, AND THE HIGHER MENTAL PROCESSES (EXPERIMENTAL)[1]

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I. Memory and Imagination

a. *Summaries and Systematic Treatises.*—Several systematic treatises of the general problems of memory and imagination have appeared during the past year. Peillaube's book (33) is the most pretentious contribution of this group; it is essentially a résumé of the familiar phenomena of this field, with an occasional reference to the traditional theories. Its most distinguishing feature is its discussion of affective memory. In common with many other French psychologists, the author assumes the existence of affective imagery. These images he discusses in detail, describing their nature, their typical forms, and their individual variations. He ascribes a prime significance to this group of images since they constitute the basis for the existence of emotional and temperamental types. Renda (38) insists that the process of forgetting is not a merely accidental character of mental function. It is the result of an active process of dissociation; and it furnishes a means by which consciousness rids itself of excess and redundant material. It is analogous with the principle of natural selection in biology; and it is a *sine qua non* of mental evolution. Piéron's book (36) treats of the problems of memory and imagination in an exceedingly comprehensive fashion; and Piéron makes extensive use of the results of other investigators in these and adjacent fields. He brings the phenomena of memory into relation with the general phenomena of retention in inorganic as well as in organic nature. The author even points out that the Ebbinghaus law of forgetting is analogous to the law of auto-catalysis in chemistry. From these inorganic beginnings, the evolution of memory is traced through the animal series to man. The culmination is reached not in the individual but in society, where, as a result of the symbolizing and schematizing function of language, a social memory is evolved.

[1] This summary was prepared with the assistance of Miss Carolyn Fisher. Several papers which have appeared during the past year are first instalments of more elaborate studies. On account of the limitations of space it is necessary to defer consideration of these until the final instalments appear. A special review of Miss Gamble's monograph will be found on p. 254.

Wreschner's monograph (54) and Lipmann's article (26) are essentially summaries of the literature; and Forel's paper (14) is an appreciative review of the work of Semon.

b. *Discussions of General Questions.*—In Balaban's investigation of the relation between mechanical and logical memory (2), lists of words were learned, and retention was tested, chiefly by means of the *Treffermethode.* In one instance the words were learned in an isolated and purely mechanical fashion; while in another, the learner was instructed to establish some sort of associative bond between the pairs of words. Tests of immediate reproduction showed that logical or associative learning was approximately eight times more effective than mechanical memorization. The introspections revealed the number of associations that had actually been established, —accidentally and involuntarily in mechanical learning, deliberately and voluntarily in the associative procedure. And these data enable the author to state that seventy-five per cent. of the associated words were remembered, while only three per cent. of the non-associated words were retained.

Starting out from the observation that a group of relatively dissimilar and complex data may be remembered better than a group of uniform and simple data, Peterson (35) proceeds to examine the inverse relation which obtains between the factors of simplicity and complexity in memorial material. His procedure consists in learning and recalling nonsense-syllables, words, geometrical figures, and the like, which in successive acts of memorization appear in different sizes, in different colors, in different spatial arrangements, etc. He finds that in those cases where meaning remains unchanged, the introduction of a few variations proves to be advantageous; but when the number of variations is increased to three or four, the advantage disappears or even becomes negative. The phenomenon that progressive increase of complexity ceases, at some point in its course, to increase memorial efficiency is probably due to distraction. The author suggests that the better retention of concrete as compared with verbal material may be a product of a more variable and extensive stimulation in the former case; for while verbal material arouses a richer ideational supplementation, it seems clear that, in most learners, imagery plays a less prominent rôle than sensory stimulation.

c. *Imagery.*—Martin (27) aimed to compare movement images which arise when the moving member is seen, and when it is not seen. Her results show that the crucial hypothesis of Müller and Schumann

—the hypothesis that one's idea of movement is most intensive when one sees the moving member—is not universally valid.

Netschajeff (29) is interested in obtaining data regarding the stock of concrete imagery possessed by children. Lists of words were presented to twelve children of about twelve years of age,—each list containing words which were calculated to arouse two visual images, two auditory images, two abstract ideas, two olfactory images, two gustatory images, etc. (!). The boys' memorial types were revealed by their answers to the questions: "Do you learn best by reading silently, or aloud?" "By visual or oral instruction?" (!). The work of van der Noot (49), and of Weil and Nellen (51) is equally unconvincing.

By an exceedingly ingenious arrangement of her experimental conditions Perky (34) succeeded in showing that the perceptual consciousness is not essentially different from the imaginal consciousness, and that an unsuspecting observer wholly fails to distinguish between the two. A detailed examination and comparison of images of memory and images of imagination reveals the following differences. (Visual) memory involves eye-movements and general kinaesthesis, imagination involves steady fixation and lack of kinaesthesis. The former are scrappy, filmy and give no after-images, while the latter are substantial, complete, and may give after-images. The mood of memory is that of familiarity and recognition, and is intrinsically pleasant; the mood of imagination is that of unfamiliarity and novelty, and is intrinsically unpleasant. Memory implies imitative movements, and the correlated organic sensations; imagination implies kinaesthetic and organic empathy. Memory images arise more slowly, are more changeable and last less long than images of imagination. Memory implies roving attention and a mass of associative material, while imagination involves concentrated and quasi-hypnotic attention, with inhibition of associations.

The papers by Abramowski (1), Betz (3), Dugas (12), Moore (28), Peillaube (33), and Piéron (36) also contain discussions of the nature, function and behavior of imagery.

d. *Association.*—Joussain (20) argues that what is usually called an associated train of ideas is rather a metamorphosis, in which a mental content passes through a continuous series of transformations. Every such transformation involves both conservation and creation,—the persistence of old elements, and the appearance of new additions to the complex. The process of transformation is not fortuitous, but is subject to definite laws,—the survival and condensation of

past experience determining the direction of transition from one idea to another.

Ohms (31) attempts to devise a method for the measurement of *unterwertige* associations. He exposes a forgotten datum, and determines the time which is required for its cognition,—the assumption being that the process of cognition will be slow in proportion as the *unterwertige* association lies far below the limen.

Bovet (6) and Kent and Rosanoff (22) have investigated the problem of preferred associations. The phenomenon that different individuals respond to a given stimulus-word with a variety of reaction-words raises the question: What proportion of reaction-words is common to a group of reagents, normal or abnormal? Bovet's paper is a preliminary report dealing with the technical problem of determining the coefficient of community. The investigation of Kent and Rosanoff is much more thoroughgoing. These authors employed one hundred stimulus-words, and one thousand normal reagents; their results are compiled in a series of tables which show the frequency of recurrence of each reaction-word. Their discussion of individuality *versus* community of association is exceedingly interesting and timely, and their frequency tables will prove to be a valuable icompend for readers and investigators of this topic.

In Langfeld's study of association (24), pictures of fimiliar objects served as stimuli; and the instructions to reagents differed from those ordinarily given in that the response must not contain the name of the pictured object. Langfeld found that negative instructions are effective in determining associations,—*i. e.*, the *Aufgabe* may be negative as well as positive. Suppression is subject to training, and may be developed by practice.

Rusk (41) reports that there is no discoverable correlation between age and rapidity of association. He believes that he is justified in concluding that imagery is not correlated with efficiency, and that thought without imagery is possible.

Jung's paper (21) is a translation of three lectures which describe and illustrate the general principles of the association method of diagnosis, and the light which this method throws upon certain constellations and upon the mental life of the child.

Nunberg (30) finds that expressive movements of the arm and hand are more intensive in 'complex' associations than in indifferent associations; yet he finds that the amplitude of respiratory movements is distinctly decreased by the presence of a 'complex.'

Franz (15) reports that the association method of diagnosis proved

to be inadequate in the case of one of his patients. A subsequent interview showed that the patient had predetermined his associations, and had thereby defeated the purpose of the test. Franz insists that an interpretation of the results obtained by this method is impossible unless the attitude of the patient be known and appreciated. The association method is not, as is ordinarily supposed, an objective method preëminently; the subjective factor is so important that the method must be supplemented by introspection if reliable results are to be obtained.

Ritterhaus (39) also casts suspicion upon the practical applicability of the association method. Jung, indeed, has pointed out that the method is a delicate instrument which can be of service only in the hands of the experienced practitioner; but Ritterhaus shows that, even in the hands of Jung himself, wholly erroneous results and unwarranted inferences have been obtained. The author gives a detailed report and analysis of his own experimental findings, and shows that any positive interpretation of these data is exceedingly problematic.

e. *Learning and Forgetting.*—Starch (44) recommends practice in the acquisition of motor skill as a laboratory exercise, and discusses the curve of learning. Dearborn (11) describes a number of ingenious experiments which are designed to throw light upon the process of learning. Book (4) shows how a knowledge of the details of the *Lernprozess*, and of the optimal periods of work and rest may be of advantage to the teacher. Eberhardt (13), himself a professional violinist, describes a series of violin exercises, by means of which technical skill may be more readily acquired. The author hints that he has discovered the secret of Paganini. The method consists in imagining the desired movement, and in concentrating the attention upon the *innerliche Vorstellung.*

Swift (45) reinvestigates the plateaus in the curve of learning, in an attempt to throw light upon the conditions which give rise to them. He fails to agree with Book in referring them to lapses of attention and effort. He reaffirms his former view, and points out that a period of mental reconstruction is attested by the fact that skill may advance during periods of cessation from practice.

Ruger (40) attempts to "analyze human methods of meeting relatively novel situations and of reducing their control to acts of skill." Twenty-seven individuals, representing various degrees of mental training, solved mechanical puzzles, and described their procedure. Many instances of human learning were found to be char-

acterized by features which are usually ascribed to animal learning alone. The paper contains a detailed discussion of methods of learning, and of the phenomenon of transfer.

Libby (25) reports that the sentence-method of learning a foreign language possesses certain advantages over the word-method. In an investigation of the act of translating Latin words and sentences into German, von Wartensleben (50) finds that while imagery may be present, yet it is to the *Bewusstseinslagen* that the greatest significance attaches in the process of translating. *Bewusstseinslagen* of familiarity, and of understanding or meaning are most important. Both the *Bewusstseinslagen* and the imagery are subject to wide individual variation; and both may be wholly lacking,—in which latter case the translating is done 'mechanically.' With increase of skill in translating, imagery tends to decrease, and *Bewusstseinslagen* to increase, in frequency.

Knors (23) finds that attempted recitation is advantageous in learning because the effort to recall a datum tends to a greater concentration of attention upon that datum, because recitation shows the learner which data are most in need of attention, because it reduces the fortuitous influences which impede passage from one datum to the next, and because it minimizes the novelty and unfamiliarity of the data.

Abramowski (1) investigated the familiar paradox which consists in our being unable to remember what a forgotten datum was, while still being able to remember what it was not,—or, as the author expresses it, the lacuna in memory resists being filled by a false datum. Pictures and words were shown to an observer, and after he had all but forgotten them, other pictures and words, more or less similar to the originals, were introduced for recognition, in order to test the observer's power of resistance to a graduated series of suggested ideas. It was found that forgetting without resistance is a rare phenomenon, and that resistance may exist in both positive and negative form. Abramowski refers the phenomenon to 'generic feelings.'

Dugas (12) reports a case of *déjà-vu* which concerns a total situation, and which ensues in a prevision of the ultimate issue of the situation, *i. e.*, the observer expects that certain events will now happen as they happened on the previous occasion. The author appends a detailed analysis and interpretation of the phenomenon.

Swift's study (46) is concerned with the reacquisition of a motor habit which had originally been acquired six years before the present

study was undertaken. It was found that much of the former skill had been lost, but the process of relearning was exceedingly rapid,—representing a saving of approximately seventy-five per cent.

f. *Unusual Memories.*—Hennig (17) describes his own remarkable memory for numbers. As reported elsewhere[1] he possesses a 'number form' which proves to be a valuable aid to memory. He now describes the affective tones which attach to dates; and reports that 'sympathetic' dates are remembered much better than 'unsympathetic,' and the latter, in turn, better than 'indifferent' dates. Schmutz (42) has accumulated data bearing upon adults' remembrances of experiences of their childhood. Joteyko's paper (19) is a description of certain mental characteristics of Mlle. Uranie Diamandi, a sister of Pericles Diamandi, and herself a 'rapid calculator' of more than ordinary ability.

II. Intellectual Processes

Since his earlier findings in the investigation of the 'indifference point' may have been the product of his experimental method, Hollingworth (18) now chooses another method and attacks the problem anew. The law of 'central tendency' in judgment is again found to be in operation; and the author suggests that it is but a particular instance of a very general phenomenon,—a phenomenon which is exemplified, for instance, in our tendency in every-day experience to conceive of a type as representative of a group.[2]

Tomor (48) attempts to confirm the hypothesis that all mental activity is a product of the reflex movements, and that the processes of thought contain both sensory and motor components.

Dearborn's investigation (10) yields the result that an idea comes clearly to consciousness only when we innervate the muscular movements which would express the idea in words; and that even in becoming aware of the form of an object we are, in great measure, dependent upon language. Brown (7) confirms previous findings that the judgment of difference is influenced by the terms in which the problem of discrimination is formulated,—an influence which is referable to the attitude or bias of the observer.[3]

Book (5) calls attention to the fact that the *Bewusstseinslagen*, *determinierende Tendenzen*, and the like, which come into operation

[1] Hennig, R. Entstehung und Bedeutung der Synopsien. *Zeitsch. f. Psychol.*, 1896, 10, 183–222.

[2] Cf. p. 220 of the June Number of the Bulletin.

[3] See Special Review on p. 218.

during more advanced stages of practice, are simply the developed forms of the images which had served as directing agents during the earlier stages of practice. And the author suggests that the dispute between the advocates and the critics of imageless thinking is due to the fact that each camp has confined its attention exclusively to the processes which are found at a single level in the development of mental function.

Okabe (32) finds that the genuine belief-disbelief consciousness is of relatively infrequent occurrence in every-day life; nor is it ordinarily an emotional consciousness. Belief and disbelief are consciousnesses of the same kind. They may be constituted of general or localized kinaesthesis, or of visual imagery; or they may be bound up with and incorporated in a particular consciousness of a verbal or visual sort. In the former case, the contents appear specifically as beliefs; in the latter case they appear as the vehicle of belief which itself finds conscious representation only in the course or mode of occurrence of the contents.

III. Practice, Transfer, Correlations

Thorndike (47) finds that a seven days' practice in adding results in an average improvement of efficiency amounting approximately to thirty per cent. The author calls attention to the surprising fact that such a slight training of old-established and frequently employed associations should yield so great an improvement of function; and he points out that the relation between amount of improvement and initial ability furnishes an index of the parts played by nature and by nurture in determining human achievement.

From examination papers in geometry, arithmetic and algebra, written by eighty-three boys, Brown (9) calculates various correlations of efficiency. The correlation between algebraical ability and arithmetical ability is high; between geometrical and arithmetical ability, low; and between geometrical and algebraical ability, negative. It is suggested that the close relation between arithmetical efficiency and algebraical efficiency is due mainly to general habits of accuracy, and that memory of preceding propositions is the ability most closely related to other forms of geometrical ability in school-boys,—is, in fact, the central ability. In two wholly unconvincing papers, Winch (52, 53) reports certain indications of the existence of 'transfer.'

IV. Abstraction and Generalization

Moore (28) attempts to trace out the mental contents which are involved in the process of abstraction, or the formation and development of general ideas. His method consists in presenting successive groups of geometrical figures which contain a common element, and having his observers describe their process of becoming aware of the recurrence of the common element. Abstraction is found to contain four stages: (1) The group is broken up, and the common element is selected, (2) perceived, (3) retained, and (4) recognized. Moore reports the existence of 'mental categories' which represent objects. These 'mental categories' are neither imaginal nor affective; they are to be regarded as 'the results of past experience'; and they constitute the essence of abstraction.

Schwiete's problem (43) is briefly this: In what form do general ideas or verbal meanings appear to consciousness? And his procedure consists in asking his observers to ideate the meanings of stimulus-words, and to report their experiences The author's conclusions seem to be as follows: There is no peculiar type of idea whose function it is to signify or represent a concept. The method of accurately envisaging a general idea varies from individual to individual. The consciousness of familiarity constitutes the first step in the apprehension of a general idea; and the vehicle of the familiarity-character may be a verbal image, a constellation of ideas, a logical context into which the word fits, or an affective experience. When the character of familiarity is intensive it may constitute the sole representation of the word. The envisagement of the word may be mediated directly by an image, by a seriation of the word into a familiar logical context or into a constellation of ideas, or by a process of defining.

Piéron (36) describes several attempts to recall names which he had, for the moment, forgotten. He emphasizes his alternations of hope and despair as he alternately felt that he was on the verge of success, or felt that the search was hopeless. He conjectures that these 'intellectual feelings' may be sensations which result from facilitation of cerebral functioning (feelings of comprehension, of recognition) or from inhibition of cerebral functioning (feelings of unfamiliarity, of obscurity).

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SPECIAL REVIEWS

MEMORY

A Study in Memorizing Various Materials by the Reconstruction Method. ELEANOR A. MCC. GAMBLE. Wellesley College Studies in Psychology. No. 1. PSYCHOLOGICAL MONOGRAPHS. Vol. X, No. 4, 1909. Whole No. 43. Pp. xii+210.

In this Monograph Miss Gamble reports a heroic set of experiments, covering many years, on memorizing the order of series of smells, colors and nonsense-syllables. The reconstruction method as she used it, was as follows: The subject is given the series, one member at a time, at a fixed rate—for colors and syllables, 3½ or 4 seconds apart; for smells, 5 to 5½ seconds, or longer if the subject needed more time to get an impression of the smell. He is then given the members in a chance order and required to assign to each its proper position in the series (by putting the bottle or card in its appropriate spatial relations to all others). One examination only of a member was allowed. A 'correct' reconstruction or ordering was one in which the positions were finally correctly assigned. A 'perfect' reconstruction was one in which this was done and in which also (1) no members had been transposed in the course of the arrangement, (2) no members which came next to each other in the series had ever been separated by a gap and (3) no members not belonging in direct sequence had ever been placed in direct sequence. If the series was not, after one presentation, 'reconstructed' perfectly, all the members were again presented to the subject in the proper order and a second attempt at reconstruction was required as before. This continued until the subject made a 'perfect' reconstruction.

The demand made in this reconstruction is like that made in memorizing a vocabulary or other set of paired associates, the difference being that the member of the pair to be remembered is the position of the given member in the series, so that remembering the associate of any one member helps (a false memory of it hinders) one in placing the others.

Miss Gamble finds that, as compared with learning to give *the elements of a series and* their order, "a relatively small number of presentations of the series was required even at the outset and apart from the effect of practice, "practice had a continuous and marked effect" throughout, and "the increase of difficulty with increase of

series-length was very small as compared with the increase found by Ebbinghaus."

The author examines by various experiments the following possible explanations of the greater ease of reconstruction, that is arranging properly the members or 'links' when given, than of *producing and* arranging them. (1) Less is to be done, the latter requiring both summoning and arranging, (2) the associative bonds may be toward a spatial localization, (3) each presentation is followed by a test, (4) ample time is given in the tests, and (5) more time was given between the members of one series, and between one series and the next.

She concludes that the common tendency to assume that the first of these is an adequate explanation is unjustifiable, since in fact the subjects do in large measure learn what the members of a series are in the course of learning to arrange them. Her evidence concerning the relative magnitude of these five factors is too intricate for summary, but should be studied by every one interested in memory from whatever point of view. Many important facts appear in addition to those that directly concern the problems which she set herself. For example, it appears that learning to produce and arrange a nonsense series is facilitated by associating each member with a place or number designating its position in the series in addition to associating it with its precursors and sequents in the series.

It is perhaps unfortunate that Miss Gamble followed the tradition of measuring and comparing the energy expended in memorizing in terms of 'repetitions,' 'recitations, and 'tests,' rather than in terms of time, supplemented by strain or discomfort. The differences between her findings and those of Ebbinghaus, Müller and Schumann, and others are in large measure due to the fact that a repetition (of hers) is not equal to a repetition (of theirs).

Her own results form a very strong argument against the use of a repetition or a recitation as a unit of measure. The fact that the number of repetitions required for long series, say of from 18 to 81 elements, did not increase as it was found to do by Ebbinghaus and others, is explained as chiefly a result of the slow presentation, and the long intervals between repetitions.

It would be interesting to try learning nonsense-series of different lengths as reversed reconstruction-series, letting the associations be 1 (or the certain spot) *zeb*, 2 (or the neighboring spot) *kag*, 3 (or the next spot) *lef*, etc., etc., and the task be to summon *zeb*, *kag*, *lef*, etc., when 1, 2, 3, etc., are given, and to compare the results, on the one

hand with learning to reconstruct the series (that is to summon 1, 2, 3, when *zeb*, *kag*, *lef*, etc. are given, all other conditions of the experiment of the experiment being identical) and on the other with learning the series by merely rereading them.

Incidentally evidence was secured to show that the degree of ability to image smells bore no considerable relation to ability to remember the series containing them, that the exclusion of secondary associations and other means of adding significance to the bare series of smells, colors and nonsense syllables is practically impossible unless the series are presented very, very rapidly or the experiment is wilfully turned into an experiment in inhibiting all natural movements of the mind; that variations in efficiency in memorizing are related to variations in amount of mental work done without rest chiefly through the variations in interest caused thereby.

Of dependence on verbal rather than concrete imagery the author notes that "The figures at their face value seem to show that the subjects who depend most on concrete imagery learned worst." The complexity of the conditions, especially degree of practice, prevented a precise estimate of the correlation. Space forbids mention of others of the many noteworthy observations.

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TACTUAL SPACE

Die taktile Schätzung von ausgefüllten und leeren Strecken. HELEN DODD COOK. Reprint from the *Arch. f. d. ges. Psychol.*, 1910, 16, 3 u. 4 Hft. Pp. 130.

All tests were made with 'passive' stimulation, i. e., the two extents for each comparison were applied to the passively resting arm of the subject, all movement of the arm itself or of the stimulating object along the skin being excluded. The stimuli were applied longitudinally on the volar surface of the forearm, about in the middle. Subjects were kept, where possible, in ignorance of the results, methods etc. In all, sixteen subjects were used. In each test the subject was to compare the length of a '*Vergleichsstrecke*' (V) with that of a '*Normalstrecke*' (N.).

PART I, *the 'successive' form of 'passive' procedure.* The stimulating points were hard rubber pegs, each tapering to an area 1 mm. in diameter; for 'filled' (*ausgefüllte* = A) extents the distance between centres of the pegs could be varied from .5 to 3.5 cm.; for unfilled

(*leere* = L) extents only the end-pegs were, of course, utilized. The stimulus was applied by means of a handle held by the experimenter, the two extents to be compared (N ranging from 4 to 14 cm.) being given successively, the second parallel to and about .3 mm. distant from the first. The method of minimal changes was employed. The chief result was that all A-extents from 6 to 14 cm. were, in comparison with L-extents, *underestimated*, and this whether the time order of application of stimuli was NV or VN. Introspection showed that the illusion was much influenced by the subjective distinctness or insistence of the 'filling-points' (*Füllspitze*) and attention to the end-points, the former disturbing the latter so as to conceal or minimize the felt difference between the limiting points. Further, whereas Rieber had found that the A-extents, although underestimated when short, were overestimated when long, Cook found no such reversal of the illusion at all. Rieber had explained the underestimation by a 'fusion' of the points when near together, but Cook shows that the maximum underestimation coincides with the maximum subjective distinctness of the points (2 cm. apart).

PART II. In this second and weightiest part of her experiments Cook used an aesthesiometric balance, one arm carrying the aesthesiometer proper, overbalanced, on the other arm, by weights in the balance pan which, when released, allowed the stimulating points carried by the first arm to descend, until mechanically arrested, with the required pressure on the arm. The points were gauged Chinese pig bristles from .105 to .110 mm. diameter. By special devices these were each vertically adjustable so that the whole row of bristle points could be set to conform to the shape of the volar surface of the forearm, and horizontally adjustable so as to secure any desired distance between bristles. Furthermore, any of the bristle holders could be, either singly or in groups, eliminated from the stimulus row, thus allowing, for successive stimuli, a fairly quick shift (6 to 10 sec.) from an A to an L or vice versa. The arm in this series lay in a plaster form and a row of about equally sensitive points, 5 cm. apart and parallel to the longitudinal axis of the arm, was marked for stimulation.

The 'simultaneous' form of 'passive' procedure. N and V were simultaneously applied, not side by side, but end to end ($\overset{A}{\cdot}\!\cdot\!\cdot\overset{L}{\cdot}$). The method of constant stimuli was employed. The subject was required to say whether V was larger than, equal to, or smaller than N. The N-extents were 4, 6 and 8 cm. in length and the points .5, 1 and 2 cm. apart. *General results*: (1) The AN-extents were over-

estimated, the LN underestimated, i. e. an A-extent was always overestimated; (2) this overestimation of A-extents did not result from visual images (*Gesichtsvorstellungen*) but, since it was just as great in two blind subjects as in strongly visual subjects, it was a tactual phenomenon; (3) if the attention was more strongly focused on one of the extents it was, in comparison with the less strongly focused, overestimated; (4) if the basis of judgment was the immediate impression of extent (*unmittelbarer Streckeneindruck*), the amount of illusion increased with increasing length of N; when other factors entered, the reverse was the case; (5) the influence of the number of 'filling-points' was not immediate nor uniform for different subjects but depended on how the extents were apperceived (*Auffassungsweise*). The constant overestimation of the A-extents in this group differs again from the results of Rieber, who found underestimation for short distances and overestimation for long; it was this particular form of simultaneous stimuli (end to end extents) that Rieber used.

The 'successive' form of 'passive' procedure. Practically the same procedure as in the experiments of Part I except that the aesthesiometric balance and the method of constant stimuli were used. *Results*: (1) an A-extent was, compared with an L-extent, underestimated. This result agrees with that in Part I above and both differ from the results of the foregoing 'simultaneous' stimulation. *The direction of the illusion appears, therefore, to be dependent on the manner in which the stimuli are presented;* (2) the illusion was greater when the 'filling-points' were closest together (.5 cm.). This fact does not, however, thinks Cook, argue in favor of Rieber's theory of 'fusion' (see Part I) and against her own result that the degree of illusion of this type increases with the distinctness of the points, since introspection showed in this case that the points most distinctly felt were those closest together. In short, the distinctness of the points is not uniformly a function merely of their increasing distance apart; on what it does depend is not yet determined.

Combination of the previous two procedures. In this group the stimuli extents were also successively applied but were spatially placed end to end with, however, a space of .5 cm. between their adjacent ends, so as to avoid stimulating the same end point twice. The result was that when an L- and an A-extent were compared, there was either no illusion, or one quite minimal. This result, along with the previous findings with successive stimuli, *shows that in successive stimulation with* A- *and* L-*extents the advantage of* A *as an impression of extent* (Streckeneindruck) *over* L *is greatly reduced.*

It is a matter of regret that space precludes further discussion of this careful, and, in inferences, conservative piece of experimental analysis. The author reaches the general conclusion that the distribution of the attention between the two extents or within either extent doubtless most influences the judgments; she represents essentially, therefore, an '*Aufmerksamkeitstheorie*.'

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MENTAL HEALING

Three Thousand Years of Mental Healing. GEORGE BARTON CUTTEN. New York: Charles Scribner's Sons, 1911. Pp. viii + 318.

President Cutten has given us in this book a most extensive and excellently arranged compilation of the phases and practices of the healing art which we now know as 'mental.' The volume is not for the specialist in these matters, the text being predominantly expository and "dealing more especially with the historical side of the subject."

Stating the common law underlying all forms of mental healing to be: "My body tends to adjust itself so as to be in harmony with my ideas concerning it," and pointing out that there is no 'best form' of mental healing, the author proceeds to show that the diversity of means employed throughout the ages has been such as to meet the wide diversities of custom, belief, and inherited superstition that at any given moment were prevailing.

From the early practices in Egypt and in India, among the Jews and among the Greeks, the reader is carried through successive ages down to the various cults of the present day. In general the association of mental healing with religion is held to have been advantageous, since "the devout attitude of mind is one most favorable for the working of suggestion." The twofold influence of the church, on the one hand in increasing human effort in alleviating illness and on the other in perpetuating the easy superstitions of mankind and thus delaying the development of medical science, is well set forth. The non-religious 'healer,' it is pointed out, has been more ready to recognize the limitations of his power and has thus been less obstructive to advances in the healing art than his confrère who has worked in the name of religion.

Separate chapters—and these perhaps the best of the book—are devoted to 'Relics and Shrines,' 'Talismans,' 'Amulets,' and

'Charms.' The material is presented with much fulness but always with discrimination and clearness of exposition. The reviewer is acquainted with no other book from which one may so readily gain an adequate impression of the controlling sway among all peoples at all times of formularies and ceremonies, of incantations and exorcisms, and of all the practices which reveal the trusting faith in the benign power of inanimate objects, nauseous and revolting doses, and absurd rites. Such an historical survey should certainly be read by all those—and there are still not a few of them—who regard the various healing cults as distinctly modern innovations.

The value of the book lies largely in its compilation and convenient arrangement of scattered material and in its marshalling of curious recipes, half sensible or utterly grotesque, to which the credulous afflicted of the past have had recourse. The absurdities of the present may thus be brought into direct comparison, in the mind of the reader, with the (happily) greater absurdities that have been outlived.

The volume closes with chapters, mostly biographical, entitled 'Mesmer and After,' and 'The Healers of the Nineteenth Century.' There is an excellent index.

A. H. P.

PSYCHOLOGY OF ADVERTISING

Principles of Advertising: A Systematic Syllabus of the Principles of Advertising. DANIEL STARCH. Madison, Wis.: The University Coöperative Co., 1910. Pp. 67.

The content of the volume is adequately expressed in the subtitle: it is thoroughly systematic and it is merely a syllabus. It is a work on applied psychology. There still survives among psychologists a prejudice against applied psychology. This prejudice would be justified if 'applied' psychology meant merely the attempt to apply established psychological laws or principles to such things as teaching, healing and selling. In so far, however, as applied psychology advances the science of psychology it should be given a place of honor by the professional psychologist. It is the conviction of the reviewer that applied psychology has demonstrated its scientific value. Educational psychology has advanced our knowledge of the processes summarized under the headings of memory and of the learning processes. Psychotherapy has stimulated research on the subjects of suggestion and the relation of the mind to the body. Animal psychology has made distinct contributions to the study of

instinct and the genesis of the higher mental processes. The psychology of religion has given incentive to a study of adolescence and of conversion. Psychiatry has increased our knowledge of the emotions and of personal identity. The psychology of advertising is throwing light upon the subjects of attention and of persuasion.

Starch's book is divided into two parts. The first part (the first 41 pages) is devoted to a study of *attention*. This part has 139 divisions or subdivisions and under most of the subdivisions references to literature on the subject are cited. The following quotation (references omitted) is typical:

"5. Law of Repetition. Attention value of an object depends upon the number of times it comes before us, *i. e.*, on repetition.
(*a*) How frequently should same advertisement be repeated?
(*b*) Is it better to use large space occasionally or small space frequently?
(*c*) How much of an advertisement should be repeated, or be in common to all advertisements of a given article? Should advertisements of a given article be placed in the same position in a given medium?"

Part II. is entitled 'Securing Persuasion and Action.' This part is subdivided into 160 sections or headings of which the following are typical examples (subheads and references omitted):

"4. Relative persuasive power of the different types of argument.
5. Motives appealing to women.
6. Appealing to instincts.
7. Securing action through imitation.
8. Securing action through suggestion."

The syllabus is intended as an outline for a lecture course in which collateral reading, personal observations and experiments are required. Although such a course as here outlined might be particularly adapted to the needs of students in a school of journalism, the course is found to stimulate interest in psychology among regular college students more readily than a similar course in pure psychology.

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BOOKS RECEIVED DURING JUNE

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PILLSBURY, W. B. *The Essentials of Psychology.* New York: Macmillan, 1911. Pp. ix+358. $1.25 net.

CRAMPTON, H. E. *The Doctrine of Evolution: Its basis and its scope.* New York: Columbia Univ. Press, 1911. Pp. ix+311. $1.50.

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MYERS, C. S. *An Introduction to Experimental Psychology.* Cambridge: The University Press, 1911. Pp. vi+156. $0.40.

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PSYCHOLOGICAL BULLETIN

GENERAL REVIEWS AND SUMMARIES

RECENT LITERATURE ON THE BEHAVIOR OF THE LOWER INVERTEBRATES

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Protozoa.—The paper by Day and Bentley (5) is on the same general subject as that by Stevenson Smith which appeared in the Journal of Comparative Neurology and Psychology in 1908. The results stated in these two papers are mutually confirmatory and the experimental methods used are essentially the same. The problem set for the paramecia was to get out of a tube so small that they could not turn around. There can be no doubt, judging from the records presented, that the time required to accomplish this feat decreased rapidly. The authors (p. 73) comment on this result as follows: "It appears from the experiments (1) that paramecium is capable of modifying within a few minutes its usual avoiding reaction; the lateral turn is so increased and prolonged as to permit the animal to reverse its long axis in a narrow circular tube; (2) that the effect of this modification remains for some time independently of external changes induced. Observations of this nature are taken as evidence of learning. . . . Nevertheless, it is questionable whether this kind of 'learning' involves consciousness; whether it is not as well interpreted as the result of purely organic processes."

Mast's paper (13) consists of two parts. The first is devoted primarily to the question of orientation, the second to the question of the stimulating efficiency of different colors. To the comparative psychologist the following quotation contains perhaps the most interesting point in the paper. "By referring to Figure 1 it will be seen that after one pseudopod came in contact with the illumination

and was stopped, the amœba did not at once proceed in the opposite direction so as to avoid the light, but sent out other pseudopods at only a slight angle with the first, apparently trying to get around the obstacle in this way. The character of the response did not change after the first pseudopod came in contact with the light, nor did it change after the second and third came in contact with it. But after the fourth became exposed the direction of motion was nearly reversed. This indicates that the reaction was modified, that the response to a given stimulus depends upon the preceding experience" (p. 267). Whether or not such changes in reactions can be considered as evidence of learning I am unable to say.

The author finds that certain amœbæ under certain conditions orient, though not very accurately, and move in a general direction from the source of light; and that a sudden and sharp increase of light intensity causes a cessation in movement. He maintains that orientation is due to the inhibition of the formation of pseudopods in the more highly illuminated side of the animal, which is in all probability caused by change of intensity, and that there is no evidence whatever that orientation is regulated by direction of the rays through the tissue, as demanded by Loeb's first theory of orientation, or the angle between the rays and the surface, as demanded by his second theory, or by difference of intensity on symmetrically located sensitive elements, *i. e.*, by stimulation of symmetrically located sensitive elements by light continuously in proportion to the absolute intensity on them, as demanded by his third and last theory.

Both prisms and filters were used in the experiments on color. The responses to the monochromatic light and to the impure colors produced by the filters were essentially alike. Violet, green, yellow, orange and red had only very slight effect on the movement of amœbæ, but sudden exposure to the blue caused instant cessation of movement. "The response to the blue is so striking and that to the other colors so very slight that there can be no question as to the specific effect of the blue (430–490$\mu\mu$). That is, the effect of different parts of the prismatic solar spectrum on Amœba is not proportional to the energy contents, for the energy gradually increases as one proceeds from the violet toward the red end, whereas the region of maximum stimulation for Amœba is in the blue, from which it decreases toward both ends. Nor it is proportional to the brightness as judged by the human eye, for the yellow is much brighter than any other part of the spectrum" (p. 274).

Loeb and Maxwell (12) tested the responses of the following organ-

isms to different colors: *animals:* Daphnia, and nauplii of *Balanus perforatus* (positive and negative); *plants:* Chlamydomonas. Nothing is given regarding methods except that both prism and Rowland gratings were used to differentiate the rays. It was found that "the strongest gathering of [Daphnia] occurs in the green," that of Balanus (positive) in the green toward the yellow, that of Balanus (negative) in the green, red and violet, and that of Chlamydomonas in the green. Regarding Chlamydomonas the authors say (p. 197), "We have repeated this experiment with absolutely the same result on nine different days." It is hardly probable that the term 'absolute' is used here in its generally accepted sense. At any rate I have never found a group of organisms that would respond to a given stimulus *absolutely* the same on nine different days.

The authors also state that acids, especially CO_2, produce "positive heliotropism in fresh-water animals (Daphnia and Copepoda) and free swimming algæ (Volvox)" (p. 197). They conclude, "It is therefore obvious that, as far as our experiments go, the heliotropic reactions of swimming animals are identical with those of swimming algæ" (p. 197). How much evidence have we here indicating that 'heliotropism' in plants and animals is identical? I am not certain just what the authors mean by 'heliotropism,' but the senior author usually uses this term to indicate orientation in light. Assuming that this is meant by the term in the paper under consideration, I cannot see how the results necessarily have any bearing on the question involved, for in no instance was it ascertained whether the aggretions in the spectrum were due to orientation or to random movements and avoiding reactions, as is the case, *e. g.*, when Chlamydomonas, Euglena, and other similar organisms collect in a vertical beam of light producing a bright spot in a dark field. But even if the aggregations in the spectrum were due to orientation in all instances, the results would have only a very insignificant bearing on the problem stated, for surely the fact that Chlamydomonas, an organism which is on the border line between plants and animals, responds to the different rays of the spectrum like Balanus and Daphnia, and the fact that Volvox, an organism claimed by zoölogists as animal and by botanists as plant, and closely related to Chlamydomonas, is affected by acids in its response to light like Daphnia, is not very convincing evidence that "heliotropism in plants and animals is identical," especially when it is known that the process of orientation in Daphnia is different from that in Chlamydomonas and Volvox, and that gradual reduction in temperature causes both Chlamydo-

monas and Volvox, if they are positive in a given light intensity, first to become quiet and then strongly negative in the same intensity, whereas it has no such effect on the response of Daphnia.[1]

Moreover, the experimental results of the botanists with but one or two exceptions agree in showing that the region of maximum stimulating efficiency in the spectrum for all plants tested is in the violet, indigo, or blue, *i. e.*, between 390$\mu\mu$ and 490$\mu\mu$. In the most recent extensive work on this subject Blaauw (1909) maintains that the stimulating efficiency at the maximum, which is in the indigo at 465$\mu\mu$, is 2,600 times greater than it is in the green, yellow or red. Loeb and Maxwell found the maximum for Daphnia and Balanus in the green well toward the yellow, *i. e.*, between 490$\mu\mu$ and 560$\mu\mu$. It is evident then that even with reference to this point there is no general agreement between the reactions of plants and animals to light.

The statements of these authors intimating that the work of the botanists on distribution of stimulating efficiency in the spectrum must be discredited because a "majority of [these] experiments were made with the aid of colored screens and . . . the few experiments made with a spectrum gave rather indefinite results" (p. 196) is, in my opinion, not in accord with the facts. In the first place a majority of botanists used prisms to differentiate the light rays. In looking over the names of all of the authors mentioned in a comprehensive review of the recent literature on this subject I find that out of the twelve mentioned only three used screens exclusively, and that only one of these three, Sachs, would be considered at all prominent in this line. In the second place, the experimental methods in most instances were excellent. In this connection I would mention particularly Guillemin (1858), Strasburger (1879), Wiesner (1879), and Blaauw (1909), at least two of whom made use of chromatic spectral colors of such purity that the Fraunhofer lines could readily be distinguished. And in the third place, a careful reading of the original papers of these authors can scarcely fail to convince any one that the results obtained are fairly definite, conclusive, and mutually confirmatory.

Vermes.—In either of Galloway's two papers (6, 7) will be found an excellent description of the reactions of a male annelid to the phosphorescence of the female during the process of mating, and this is the only point of especial interest to students of behavior or comparative psychology. It is, as far as I know, the only instance in which

[1] See MAST's *Light and the Behavior of Organisms*, pp. 274–279.

phosphorescence in organisms has been shown to be of unquestionable ecological importance. Just before mating, which probably occurs periodically about every 26 days during several summer months, the females are found at dusk swimming about near the surface of the water. At first they are only very slightly luminous, but later they quite suddenly become acutely phosphorescent, particularly in the posterior three-fourths of the body. At this phase they swim rapidly through the water in small, luminous circles two inches or more in diameter. "The male first appears as a delicate glint of light, possibly as much as 10 or 15 feet from the luminous female. They do not swim at the surface, as do the females, but come obliquely up from the deeper water. They dart directly for the center of the luminous circle and . . . seize the female with remarkable precision, when she is in the acute stage of phosphorescence" (6, 3). "So far as could be observed, the phosphorescent display is not repeated by either individual after mating. Very shortly the worms cease to be luminous and are lost" (7, 16). The authors lean toward the opinion that the luminosity of the male is useless and that the phosphorescence is a by-product of 'a form of metabolism.' I assume they would maintain that its origin was accidental, and had nothing to do with its present function in mating.

The function of the eyes and the nature of the stimulus constitute the main problems in Mast's investigation (14). Four different species were studied. The reactions in all were essentially the same. Specimens with the eyes removed either by gouging them out or by cutting the head off responded to light after the wounds were healed by becoming more active when exposed in strong illumination, but there was no indication of orientation. Normal specimens of all four species orient fairly accurately, as did also the amputated anterior ends which contained the eyes.

These results, the author maintains, show clearly that the eyes of marine turbellaria function in the process of orientation. He also holds that the increase in activity when the illumination is increased is dependent upon the amount of light received, that is, continued intensity. It is not a response to change of intensity. This conclusion is based largely upon the fact that the increase in activity does not take place until some time after the intensity of the light has been changed. With reference to the nature of the stimulus which regulates orientation he says, "Our evidence is not conclusive, although it indicates that it is due to change of intensity" on the sensitive tissue in the eyes. This evidence may be found in the original paper.

The problems examined in Morgulis' paper (17) are: (1) Does the worm react as a succession of separate segments or as a unit organism? (2) "What determines the worm's movement in a definite direction?"

The author confirms Friedländer in his conclusion that there is coördinate movement in the two parts of worms cut in two and sewed together. He also removed a section of the nerve-cord and still found coördinate movement, but he says that if either anterior or posterior end is lightly stimulated, contraction waves passing backward or forward do not pass the 'nerveless region.' "Muscular contractions unless very strong are insufficient to induce a state of muscular activity in an adjacent part." But his main argument in favor of the idea that the worm does not "react as a chain of segments but rather as a unit" (p. 623), is based upon the observation that when the posterior end is turned to the right or left, the anterior end immediately turns in the opposite direction, while there is no movement in the central portion. This the author intimates is due to an impulse passing rapidly from one end of the worm to the other, and not to a successive action of segments. His assumption that the tendency of organisms to move in a definite way, *e. g.*, the tendency of a worm to move in a straight line, is a neglected factor in behavior, probably applies to the observations of a certain few investigators, but in my opinion it cannot be maintained to have a general application. It simply means that the behavior of organisms is not determined solely by external factors. And this idea is the keystone in the work of Darwin on *The Power of Movement in Plants*, published in 1880, and is prominent in the work of Jennings and others.

Echinoderms.—When an Ophiuroid comes in contact with a solid and is then pulled away it usually returns to the spot touched. This is among the most important points established in Cowles' interesting paper (3). It shows an apparent after-effect of the constant stimulus, a sort of primitive memory of past stimuli. The ray which made the contact acts as leader for some time after, and movement occurs in the direction indicated by this ray, unless some other stimulus, *e. g.*, strong light, acts in opposition. The author maintains that movement up vertical walls is not a response to gravity, as many hold, but is due to a tendency of continuing in a given direction, so that whenever the animal meets an obstruction it does not turn to the right or the left but goes up. The tube feet function in feeding by seizing particles and passing them toward the mouth. This occurs even in rays cut from the body. They distinguish between food and in-

organic bodies by means of chemical stimuli, for they take pellets of filter paper soaked in meat juice and reject those soaked in sea water. These animals are negative in their reactions to light. They go toward vertical walls but not toward shadows on the floor. The author does not analyse these reactions, but the movement toward dark walls is in all probability due to the reduction of light from this direction owing to the wall, while the failure to go toward the shadow on the floor indicates that the shadow does not sufficiently obstruct the light from its direction to produce a directive influence.

A second paper by Cowles (4) relates to the behavior of the starfish. Echinaster is decidedly migratory. These animals "use their tube feet in moving about over the sand in much the same manner as human beings use their legs" (p. 97). Both Echinaster and Astropecten live in regions exposed to the brightest light and they are both positive in their photic reactions. No preference is shown for any ray as leader, but orientation is more accurate when a ray is directed toward the light than it is when the region between the rays is thus directed. Removal of the eye-spots does not interfere with the accuracy of orientation. This is in harmony with the observations of Mangold (1909). A "prism of water mixed with a little Higgins water-proof ink" was arranged over a box so as to make the illumination gradually more intense from one end toward the other. The starfish persistently moved toward the brighter end. The author assumes that this proves that the direction of the rays does not control the direction of movement. The same objection however may be offered here as has frequently been offered to a similar apparatus used by Oltmanns and others. If one were to place the eye in the position of the starfish in the apparatus, it is undoubtedly true that the difference in illumination of the two ends of the prism could be clearly detected, which means that these ends act as secondary sources of light, that at the thinner end of the prism being the more intense.

Moore's paper (16) consists mainly of an attack on Jennings' position with reference to the righting reaction of the starfish. Jennings points out that the righting reaction in the starfish is a regulatory response, without which those specimens which chanced to become turned over would have to perish. He suggests that the injury of remaining on the dorsal surface may be due to several factors: (1) cessation of locomotion and consequently of "finding and capturing food"; (2) pressure of the gills on the bottom resulting in possible injury and impeding respiration; (3) displacement of internal

organs that may be "harmful to their proper functioning." He describes in detail just what occurs during the process of righting but attempts no further analysis of this response. He maintains that certain specimens which have a tendency to use a given ray constantly in initiating the righting reaction could be taught to use other rays if the tube feet of the given ray were repeatedly stimulated with a glass rod so as to prevent them from attaching.

(1) Moore fastened a glass plate so that it touched the gills of a starfish 'lightly' as it crawled under, and found that it moved right on and did not turn over. (2) He turned a glass plate to which a starfish was attached over so that the dorsal surface was down and again found that the starfish did not turn over. (3) He injured the active rays "(*a*) [by] irritating the ventral groove of the arm by rubbing it with a glass rod, (*b*) [by] treating the tips of the arm with a few drops of *n*/10 acid" (p. 239), and found that he could thus make specimens use a ray to initiate the righting reaction which they formerly did not use in this way. These results, he maintains, show that Jennings is wrong, first, in his statements as to the cause of the righting reaction, and second, in his conclusion as to the cause of change in the ray used to initiate this reaction.

He concludes (1) that the righting movements "are due only to the positive stereotropism of the tube feet," and (2) that "a starfish cannot be taught to use an arm which is ordinarily passive" (p. 239). But he maintains (p. 237) that his observations agree with the assumption of Loeb that "the mechanism of the righting movements is the result of coördinating and inhibiting impulses, which are transmitted to the various arms by the ventral nerve ring."

I am unable to see how either of the author's experiments on the righting reactions has any bearing on the position that Jennings takes as to the nature of this response; for all that his experiments teach is that this reaction is not primarily a response to gravity or to contact stimulation of the gills, and Jennings certainly does not maintain that it is. In neither of the experiments was the normal functioning of the starfish appreciably hindered, whereas the central idea in Jennings' statement is that the righting movement is a regulatory response by means of which the starfish avoids conditions which tend to interfere with the normal functions of its organs. The author intimates that Jennings injured the tube feet by stimulating them, and that the starfish used a new ray to initiate the righting reaction, not because of the experience of repeated trials but because it could not use the other rays owing to injury. I am not familiar with the

details of Jennings' experiments on this point, but it certainly seems possible to stimulate without injury the tube feet sufficiently to prevent them from attaching, and that is all that Jennings did.

Upon what the author bases his conclusion that "a starfish cannot be taught to use an arm which is ordinarily passive" (p. 239) is not clear. He says nothing about having attempted to teach one. Surely the fact that it uses a new ray when all others are injured has no bearing on the question of learning.

The author's final conclusion that the righting movements "are due only to the positive stereotropism of the tube feet" (p. 239) does not seem to be in harmony with his preceding statement that "the mechanism of the righting movements is the result of coördinating and inhibiting impulses, which are transmitted to the various arms by the ventral nerve ring" (p. 237), unless he assumes that the coordination and inhibition are due to the stereotropism of the tube feet. But what is this stereotropism which the author maintains gives a "much simpler explanation of the righting movements" than that suggested by Jennings? Unfortunately he gives us no answer to this question. "The tube feet," he says (p. 235), "are positively stereotropic. Therefore the arms twist and turn until all of the tube feet are in a position to be in contact with a surface." Is this any more than saying that the starfish turns over because it has a tendency to take a position such that its feet can function? Surely no one who has ever seen one of these animals on its back would be unreasonable enough to maintain that there is anything in the nature of an attractive force between the tube feet and the substratum that brings them together. A fly rights itself; it tends to keep its feet in contact with a solid; they are stereotropic. Are not its righting movements as fully explained as are those of the starfish, when it is said that it turns over because its feet are positively stereotropic? And the same might be said with but little modification with reference to the human being. His feet too are stereotropic; they tend to remain in contact with solids. If the author had used the expression 'tendency to come in contact with solids' in place of 'stereotropic,' he would have seen more clearly in what his simple explanation of the righting movements consists, and that the problem involved in the righting movements of the starfish is essentially the same as that involved in the question as to why the human being stands on his feet in place of his head.

Mollusks.—In his investigation on the photic reactions of gastropods, *Littorina litorea* and *Littorina rudis*, Morse (18) was unable to confirm Bohn in his conclusion that these animals change

in their response to light from positive to negative and *vice versa* in correspondence with the movement of the tides, and that they retain these periodic changes for some time after being taken from the sea and put into aquaria, exhibiting a sort of primitive memory of the tides. However this may be in regard to Littorina, there are animals in which periodic responses have been fully demonstrated to continue for some time after the periodic changes in the environment which ordinarily accompany them have been eliminated. The worm Convoluta and the crab Idothea studied by Keeble and Menke respectively are notable examples. In plants such reactions have long been familiar.

During the process of locomotion in most of the gastropods, waves are seen to pass either forward or backward over the foot. The principal point that Parker establishes in his paper (19) is that these waves consist of concavities or troughs, not convexities, as generally assumed. With this point established he sets forth a theory of locomotion which consists mainly of the idea that as vertical muscles produce the troughs by contraction, longitudinal muscles extending from the portion raised from the substratum forward to the attached portion draw the elements in the trough forward. This process repeated in succession either forward or backward results in forward movement of the entire animal. This theory is thought to apply equally to the movement of those gastropods in which no waves are seen. In these the author assumes that there are concavities in the foot and that the tissue in them is drawn forward just as in the other, but that the concavities are promiscuously scattered and consequently do not form troughs, so that it would be very difficult to see them.

Arthropods.—The prime object of Banta's investigation (1) was to ascertain (p. 439) "why the one animal, *Cæcidotea stygia*, is a cave inhabitant, while the other, *Asellus communis*, its near relative living in the same region, rarely occurs in caves." The author finds that both of these isopods tend to collect in light of low intensity, but that the surface species becomes positive in its reactions after having been subjected to total darkness for some time. The cave form is much more sensitive to tactile stimuli and vibrations and has much keener discrimination in selecting its food than the surface form. The reactions in general in both forms are highly adaptive and as such would tend to keep Cæcidotea in the caves and to direct Asellus out. The author holds that the habits and structures which eminently fit animals for life in caves are not primarily due to modifications caused by the environment in the caves, but that

the animals now existing in caves had become closely adapted for cave life before they became cave dwellers. He says (p. 485), "Since Asellus is so closely related to Cæcidotea morphologically and physiologically, it would seem that if under stress of circumstances any epigeal animal could suddenly become a cave inhabitant, Asellus might be expected to be capable of undergoing such change in environment." The paper is well written, and there is every evidence that the experimental results are trustworthy. But it seems to me that it could have been advantageously shortened by the elimination of some of the twenty-three tables, many of which cover more than a page.

Hess (8) thinks that the eyes of insects and crabs contain fluorescent substances, and he maintains that the reactions in ultra-violet are in all probability reactions to longer waves produced by the effect of the ultra-violet on the phosphorescent substance. He finds no evidence of such a specific response to the different wave-lengths as would indicate color vision.

In his paper on phototactic reactions, Jackson (11) maintains: (1) That Hyalella, which is normally negative in its reactions to light, becomes positive when dropped into weak solutions of the following substances: hydrochloric, acetic, oxalic acids; ammonium and potassium salts (bromide and iodide); ammonium hydrate, ethyl alcohol and ether. They also become positive in a saturated solution of boracic acid. Tartaric acid and the following substances produce no change: sodium and magnesium salts, potassium and sodium hydrate and potassium carbonate. (2) The strength of solution which produces reversal in reaction varies greatly with different compounds, *e. g.*, in chromic acid 0.0046 per cent. induces the change, whereas in acetic it required 0.01 per cent. and in boric acid a saturated solution. (3) When the animals were put into 25 c.c. distilled water and the chemicals were added very slowly and gradually they remained negative in all cases until they were killed.

He concludes (p. 263), "It is probable, therefore, that these various changes of reaction are due not to chemical changes in the eyes or skin of the animals, but to a sudden stimulation or shock to the nervous system." This conclusion appears to be in direct opposition to that of Loeb and Bohn on this subject, for these authors maintain that the chemicals in the environment, *e. g.*, acids, have a *specific* effect on certain assumed chemical reactions in the sensitive tissue.

McGinnis's article (15) presents the following results: Branchipus

is positive in its reactions to light. It orients with the ventral surface facing the light, no matter whether the source is above, below, or to the side. In total darkness these animals are negative to gravity and come to the surface. In diffuse light they are positive and go to the bottom.

General Papers.—It is well known that certain animals are more sensitive to a given external condition when expanded than when contracted; *e. g.*, earthworms and fly larvæ respond to much weaker illumination when the anterior end is thrust out than when it is withdrawn. Bohn (2) has made observations on a number of other animals in which he says this is true. He attempts to account for this change in sensitiveness on purely physico-chemical grounds. His preliminary assumptions are however so numerous and hypothetical that his explanation is hardly worth considering. For example, the stem of the cœlenterate Veretillum, an approximately cylindrical structure, grows down at the end when it is placed horizontally. This is due to a response to gravity. Bohn observed that in the growing end of a stem there are wave-like contractions which proceed toward the tip, consequently the tip is subjected to alternate periods of contraction and expansion. Now he finds that this structure turns down only during the period of expansion and he *assumes* (pp. 497–501), (1) that the reacting tissue becomes more sensitive during this period; (2) that the upper surface is more sensitive than the lower; (3) that the sensitiveness depends upon a chemical reaction; (4) that this chemical reaction takes place between a hypothetical substance within the tissue and some substance in the environment, *e. g.*, oxygen; (5) that the internal hypothetical substance is lighter than protoplasm and consequently collects at the upper surface. On these assumptions, for none of which, with the possible exception of the first and the third, is there the slightest evidence, he bases his explanation of the response of this organism to gravity. Over one hundred years ago (1806), Knight put forward a similar hypothesis to explain the reaction of plant stems to gravity. This hypothesis, however, has long since been abandoned. All of the other so-called physico-chemical explanations in the paper under present consideration are of the same order, and still the author maintains (p. 501) that his hypotheses "not only explain the known facts but every day lead to the discovery of new ones; they are very fertile working hypotheses, to which the study of phosphorescence . . . gives a certain reality." But what bearing phosphorescence has on these hypotheses he fails to mention. Practically every one aims at a mechanical explanation

of life phenomena and welcomes any advance along this line, but I am of the opinion that there is nothing gained by such crude attempts as have been presented in this paper.

The results obtained in Hess's work (9) have an important bearing on certain rather widely accepted ideas regarding the fundamentals in reactions to light. With the exception of Amphioxus the investigation is confined to the arthropods and mollusks, of which representatives of some fifteen different genera were tested, including the historically important forms, *Porthesia chrysorrœa* and Daphnia. The principal problem in the mind of the author appears to have been to ascertain the distribution of stimulating efficiency in the spectrum. In nearly all instances the light used was a Nernst-lamp prismatic spectrum. In such a spectrum the energy increases fairly regularly from the violet to the red end. Four different methods were employed: (1) aggregation in the spectrum, (2) movement of the eye of Daphnia, (3) contraction of the pupil in cephalopods, (4) activity of Amphioxus and mussels, especially the contraction of the siphons. In all instances the reaction to different intensities of white light was first ascertained and it was found that the reactions to the more intense light corresponded to those in the yellow-green region of the spectrum, and those to the weaker light corresponded to the reactions in the violet and red: *e. g.*, animals which were positive in white light aggregated in yellow-green; those which were negative, in the violet and the red. The author maintains, as others have shown for different organisms, that the reactions of the higher invertebrates are in opposition to the conclusions of Loeb (1) with reference to his ray-direction theory of orientation and consequently non-adaptive reactions, and (2) with reference to his dictum that the photic reactions of plants and animals are 'identical point for point.'

While Hess has shown conclusively that Loeb's experimental results do not warrant his conclusions regarding orientation (others have also pointed this out), I do not think that his evidence in favor of the opposite is conclusive, for in no instance has he eliminated the possibility of orientation to the direction of the rays. In the experiment, *e. g.*, where animals exposed in the entire spectrum move directly toward a given region, it is evident that the light reflected from that region may be the controlling factor in their movement. However, the evidence in opposition to Loeb's second conclusion is convincing, for it is well known that for most plants, as pointed out elsewhere in this review, the region of maximum stimulating efficiency in a spectrum like the one used by Hess is well toward the violet

end, whereas Hess has clearly shown that for all of the higher invertebrates tested it is in the yellow or green.

Holmes (10) after pointing out that all theories of the origin of intelligence based upon the 'psycho-physiology of pleasure and pain' are limited in their usefulness owing to our ignorance concerning the nature of physiological states which accompany given psychic manifestations, and after discussing the point of view of Hobhouse, concludes as follows: "Animals in the course of their instinctive responses encounter stimuli which bring about other responses. These become associated. *According to the nature of the nervous pathways involved, there may be reinforcement of or interference with the original reaction.*[1] Experience brings about an extension of the range of adaptations by the assimilation of congruent reactions and the elimination of acts whose secondary consequences are in the nature of antagonistic and thereby inhibitory responses. Such we may say, by way of expressing a tentative view-point, is the nature of primitive intelligence" (p. 480).

It seems to me that in the assumed nervous control of "reinforcement of or interference with the original reaction" found in this theory we have precisely the same sort of problem as that which we have in the physiological states associated with pleasure and pain, a problem which in the words of the author is 'rather a fruitless one.'

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[1] The italics are the reviewer's.

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RECENT LITERATURE ON THE BEHAVIOR OF THE HIGHER INVERTEBRATES

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Tropisms

Loeb claimed that, in water, blow-fly larvæ are positively geotropic, but not so under any other circumstances. Recently Mast (9) has conducted some experiments, in both air and water, which militate against this induction. In the air experiments, the larvæ were placed in glass jars. The small larvæ moved about in every possible direction; the larger larvæ crawled upwards, but frequently turned and started in other directions. When such larvæ approached the horizontal, they either fell to the bottom, or each time the posterior end of the body was raised in the process of looping, it swung downward somewhat, thus producing an orientation with the anterior end up. To test their reactions in water, larvæ 6 mm. long were placed in a glass jar containing water 20 cm. deep. All of the larvæ reached the bottom, but there was no evidence of either swimming or orientation; in many cases the longitudinal axis was nearly horizontal. From these experiments Mast concludes that blow-fly larvæ do not react to gravity either in the water or out of it. According to Loeb's definition of a tropism, this conclusion is sound.

Holmes (7) has conducted experiments which he thinks prove

that larval mosquitoes are positively phototactic, but that their reaction to shadows is something more than a tropism. Whenever a shadow was cast upon a larval mosquito, the insect swam downward; this was true even when the shadow was cast upwards by a light placed beneath the aquarium. Holmes considers this a specific reaction to a shadow, which reaction is tinged with a little negative phototaxis. The larvæ were found to vary greatly in their phototactic response; some were indifferent, some moved towards the light and others away from it; but all reacted towards shadows in the manner here described. There was no orientation, and, upon arrival at the positive side of the dish, the position of the body bore no relation to the direction of the rays of light. Age made little difference in reactions. Larvæ that were frequently exposed to shadows gradually ceased to respond to them. The adults, likewise, showed a combination of responses to light; but were more apt to settle on dark than on light objects, and, during the day, they sought the shade, even when it was not hot or dry. At night they sometimes flew towards the light. Some species showed marked positive phototaxis. Eating a full meal did not alter the phototactic response.

Bohn (2) found that the caterpillars investigated by him, when confronted with a cylindrical object, whether it were a part of a plant or a rod, always moved along the body in the direction of its length. When the larvæ were confronted with a series of illuminated patches, in a graded series, up to a certain point the caterpillars were positively phototactic; beyond that point they were negatively so. Parasitized and well-nourished caterpillars were negatively phototactic, while non-parasitized and poorly nourished forms exhibited positive phototaxis. According to Bohn, these tropisms force the parasitized and well-nourished caterpillars to seek dark crannies in which to weave their cocoons, and induce the non-parasitized and poorly fed to find the plants upon which they feed.

According to Andrews (1), termites, although blind, respond to light by collecting where the intensity of the light is least. Andrews does not call this a tropism, but Bohn would.

Differential Sensibility

Lately Bohn has been laying much stress upon what he calls the law of '*sensibilité différentielle*,' which law he claims to have been the first to enunciate. In a recent communication (2), he gives as types of the workings of that law the tendency, so common among many inferior animals, to turn through an angle of 180° when con-

fronted with an abrupt change in the mechanical, physical, or chemical condition of the environment. In the same communication, Bohn gives the following examples of the working of the law. When placed in a dark dish and carried into a dimly lighted room, caterpillars move towards the window. If a dark veil is held at a certain distance above a dish containing caterpillars and then suddenly removed, the caterpillars immediately scatter in all directions. When a dish is partially covered with a dark veil, the caterpillars move towards the shadow; but at the edge of the light, if the contrast is marked, they halt, deviate, or even retreat. If under the term '*sensibilité différentielle*' we include all non-orientational responses to an abrupt change in the environment (and any definition narrower than this would exclude the third example cited from Bohn), the term is synonymous with what Jennings has called 'reaction to change,' and with what Loeb has styled '*Unterschiedsempfindlichkeit.*' Although we may feel that differential sensibility, a translation of Bohn's term, makes a good English name for the phenomena, yet the credit for first stressing the importance of this factor in behavior must be given, not to Bohn, but to Jennings, who discussed the matter as long ago as 1904.[1]

Sensations

1. *Vision.*—During the past year two investigators, Turner (15) and Lovell (8), have reported experiments which they think demonstrate that the honey bee possesses color vision. Although the conclusions of these two investigators are essentially the same, their methods are different. Turner used colored discs, colored cornucopias and colored boxes, all of a small size. After a few bees had acquired the habit of collecting honey from a disc of a certain color, three different series of experiments were conducted; one with discs, one with cornucopias, and one with boxes each provided with a small opening. In each of these series a large number of artefacts of two colors, half of which were of the color of the disc from which the bees had learned to collect honey, were scattered, promiscuously, among the flowers from which the bees were foraging. The artefacts of the color from which the bee had learned to collect honey were supplied with honey, the others were not. All of the artefacts containing honey were visited by numerous bees; no bees visited the other artefacts. Control artefacts of the color from which the

[1] Jennings, H. S. The Behavior of Paramœcium. *J. of Comp. Neur. and Psychol.*, 1904, 14, 464–468. *The Behavior of Lower Organisms*, 1906.

bees were collecting honey were well supplied with honey and placed in portions of the field where the bees had not been trained to feed from artefacts. Although the bees were numerous, these artefacts were not visited. At the close of both the second and the third series of experiments, all of the artefacts were removed from the field; and two artefacts, one of each color, both new and neither containing honey, were exposed in the field. In a few minutes, the artefact of the color that had formerly marked the artefacts that contained honey was completely packed with struggling bees. No bees were in the other artefact. In each series the artefacts were distributed in both the sunshine and the shadow. All were equally visited by the bees. Since the brightness content in the two cases was different while the color was the same, it was concluded that the bees were reacting to colors as such.

Lovell trained bees to collect honey from a glass slide superimposed on a piece of colored paper. He then poured honey upon several pieces of glass, each of which rested upon a piece of colored paper. These slides were arranged in a row and the order changed from time to time. The bees collected from only those slides that covered pieces of paper of the color of the one from which the bees had been trained to feed. No attempt was made to rule out grayness as a factor.

In daylight the caterpillars studied by Bohn (2) moved towards dark bodies. When such a body was moved away, all of the caterpillars would follow it. When it was suddenly moved towards them, they often halted and made a half turn, as though they saw and were reacting according to the law of differential sensibility.

As a result of observations made on the mating of *Dysdera crocata*, Petrunkevitch (13) reiterates his statement that sight is the only means of sense-recognition in hunting spiders.

Pearse (12) has conducted a series of experiments for the purpose of determining if the reactions of protectively colored arthropods indicate that such animals realize that their coloration is advantageous on certain backgrounds, but not on others. Limited space forbids a description of the well-planned experiments. He concludes: "It cannot at present be affirmed that any protectively colored arthropod reacts towards colored objects or backgrounds in such a way that it can be said to have even an instinctive knowledge that it is protectively colored; *i. e.*, arthropods do not choose the most favorable color environment on account of color."

2. *Hearing.*—Andrews (1) furnishes evidence that termites are

affected by concussions of air, leading them to retreat to the nest after thunder, etc. He does not definitely state that they hear; but he expresses the belief that these concussions affect special organs, resembling the ears of locusts, found on each of the legs of the termites.

Mating and Nest-Building Instincts

Petrunkevitch (13) describes in detail the courtship of *Dysdera crocata*. He found that the female made no objection to the approach of the male. Vision enables the male to recognize the female and touch is the chief means he uses to incite the female and to test her willingness to accept him.

Viehmeyer (17) in the published results of some recent researches asserts: (1) *Formica sanguinea* was originally a robber ant: its slavery and its social parasitism have developed out of its robber habits and not out of a previous adoption stage; (2) the colony is founded in the following three ways, through the stealing of pupæ by the females, through alliance with subsequent stealing of pupæ, through adoption; (3) ontogenetically these three forms of colony building are an adaptation to the condition in which the colony is founded by means of slaves; (4) phylogenetically they represent stages in the degeneration of *Formica sanguinea*.

Wasmann (18) gives examples of the following six methods of colony founding by *Formica sanguinea:* (1) by stealing of pupæ by the female; (2) by the peaceful adoption of the female by old workers of the 'help-ant' species; (3) by the peaceful alliance of females of different kinds; (4) by colony-splitting (migration), *i. e.*, by the help of workers of the same species; (5) by the accidental finding of pupæ by the female; (6) by the adoption of a female by a strange colony of the same species. Wasmann defines the terms *pleometropism* and *allometropism* and makes subclasses under each; by the former he means the existence of two or more females of the same species in a nest, by the latter the existence of females of two or more species in the same nest.

Brun (3), as a result of studies of artificial and natural mixed colonies of *Formica rufa*, states that in this species new colonies are formed both by migration and by adoption, and concludes that the artificial and even the more natural alliances of the species are phenomena of complicated, psychically plastic activities, in which, now singly, now in combination, the following factors play a rôle: the disturbance of normal instincts by a powerful stimulus, the supplanting of one instinct by another, psychical contrast effects, the

rapid association of new stimuli with each other and with retained elements.

Fighting and Hunting Instincts

Wasmann (18) relates an instance of a nest of *Formica sanguineas* being slaughtered and eaten by the slaves of that colony.

Brun (3) describes a battle between *Formica rufa* and *Lasius fuliginosus* which had a remarkable ending. Workers, females, and young from several different nests of the former species were collected in a common sack and dumped near a colony of the latter. Having been defeated by the *Lasii*, the *rufas* organized an orderly retreat and, fighting as they departed, retreated in the direction of a *rufa* nest. On nearing the nest, the inhabitants rushed out, joined the defeated *rufas* and drove the *Lasii* back home. Then the miscellaneous collections of *rufas* became an integral part of the foreign *rufa* colony with which they had become so strangely allied.

Sanders (14) describes a series of conflicts between a certain nest of *Myrmica scabrinodes subuleti* Meinert and two neighboring nests of *Lasius niger americanus*. The conflict started between the *Myrmicas* and one nest of the *Lasii*, the second nest of *Lasii* taking no part until the last conflict. On the first approach of the *Lasii* a *Myrmica* rushed into the nest and returned with two companions. These three rushed into the nest and returned with an army. The *Myrmicas* formed a line of battle and soon defeated the *Lasii*. On returning to their nest, the *Myrmicas* left about twenty ants on guard near the nest of the *Lasii*. All subsequent conflicts, except the last, were essentially a repetition of this one; only each time fewer and fewer guards were left by the *Myrmicas*. At the last conflict, after the *Myrmicas* had formed their line of attack, which, as usual, was strongest in the middle, the second nest of *Lasii* approached in the rear of the *Myrmicas'* line of battle. Thus outflanked, the *Myrmicas* broke rank and rushed around the end of the *Lasii* to their own nest. Sanders thinks these conflicts show that the ants profit by experience and that the *Myrmicas* realized the futility of leaving guards to watch the *Lasii*. The first conclusion seems justified; but the latter is hardly proven.

It is well known that certain European Ammophilas stock their nests with subterranean caterpillars which they dig from the ground. The majority of the American students of this group write as though American forms never do so. Recently (16) it has been shown that some of the American Ammophilas have the same habit.

Harvesting Habits

Neger (11) has made a number of observations on *Messor barbarus* which seem to warrant the following conclusions: There is no evidence to justify the statement that ants prevent seeds from sprouting by drying them, for the seeds brought out to dry have already sprouted. The germination of the seed does not, as a rule, extend beyond the first stages; and in seeds which the ants dry in the sun there is no transformation of starch into maltose and dextrine. The seeds, which have been shelled and dried, are taken into the nest, cut into small pieces, chewed well and made into dough. This dough, in the form of crumbs, is brought to the surface and dried to the consistency of biscuits. On account of their hardness, these dried 'biscuits' seem unsuited for nourishment; hence Neger thinks they are used as a substratum upon which to grow a fungus, probably *Aspergillus niger*. This is a leaf cutting species; all of the grass and leaves near the nest are cut down and carried inside.

Miscellaneous Instincts

Morris (10) thinks that the leaf rolling of the oak-leaf rollers is instinctive, but that it is not absolutely perfect. Scattered through his paper are many notes on the habits of beetles.

Symbiosis and 'Amikalselection'

The following opinions of Wasmann (19) on symbiosis and '*Amikalselection*' will prove of interest to students of animal behavior. Symbiosis among ants occurs where the adult ants and the guest insect live together on friendly, mutually helpful terms. In symbiosis the guest exudes a product which is used by the host as food; this distinguishes it from parasitism. Although pathological conditions may sometimes occur, yet symbiosis is not a social sickness of the ant city, as Escherich claims. A sharp line must be drawn between symbiosis as such and the evil effects of its over-development. On the part of the host there are two biological foundations for symbiosis: the ant's fondness for good eating and the adoption instinct of ants. *F. sanguinea* possesses an inheritable instinct for caring for *Lomechusa strumosa*; in *F. rufa* this instinct is much less pronounced; in *F. pratensis* and *F. truncicola* the instinct is null, for they eat the larvæ of the above beetle. In opposition to Escherich, it is held that this marked difference in the behavior of these closely allied species can only be accounted for by assuming the existence of a symbiosis instinct.

While granting that personal selection and germinal selection play a small part in the development of this symbiosis instinct, Wasmann contends that the most influential factor is what he calls '*Amikalselection*,' which is defined as the positive, instinctive, selective breeding which ants and termites practice towards their true guests. According to him, the true ant guests and termite guests are a selection product of the symbiosis instinct of their host, brought about by '*Amikalselection*.' To Escherich's contention that the predication of '*Amikalselection*' is an anthropomorphism he replies that no reasoning is predicated. To Escherich's statement that '*Amikalselection*' is only a form of natural selection he replies that natural selection is a negative factor, while '*Amikalselection*' is a positive factor. As far as the host is concerned, '*Amikalselection*' works independently of, and often counter to, natural selection. So far as known, '*Amikalselection*' occurs only among the ants and termites.

Division of Labor

In a paper giving an elaborate treatment of the habits of the termites of Jamaica, Andrews (1) makes the following remarks upon the division of labor among them: "The workers do all the mechanical work; bite off and transport the wood; feed the soldiers, and males and females; clean the males, females and soldiers; remove the eggs from the orifice on the end of the female and clean and transport them; do all the work of arcade and nest building and all the biting in defense of the community. The soldiers do no mechanical work, except to move themselves; they appear first when the nest or arcade is disturbed, they explore what is novel, lead the advance of processions, stand placed like guards along the sides of processions. They respond to anything unusual rather by exploratory advances than by retreat; being the quicker moving and more responsive members of the community. They form the 'investigator' rather than the soldier caste. Yet they fight, in a refined way, by ejection of a secretion that binds the enemy fast."

As the result of a series of well planned experiments, extending over several years, on the division of labor among ants Miss Buckingham (4) reaches the following conclusions: In *Camponotus americanus* there seems some evidence of division of labor correlated with polymorphism; this is not marked by hard and fast lines, since all classes may share in any activity, but by a preponderance of large ants in fighting, of those of medium size and smaller in building, of those of small size in tending the young, and

of those of small and medium size in foraging. As a rule, the small and medium-sized workers are more active than the largest ones. In *C. herculeanus pictus* there is no hard and fast morphological or physiological distinction between the classes; but the intermediates and minors excel in household duties and foraging and the majors are relatively inactive in all occupations except fighting. In *Phidole pilifera*, there is no hard and fast line between the duties undertaken by the separate classes; but the minors show greater activity in foraging and all household duties and are more active than the soldiers. Callow ants do not fight: this, Miss Buckingham thinks, may account for the fact that the young of different species will dwell together amicably, forming mixed colonies. This is certainly a much more plausible theory than the one proposed some years ago by Miss Fielde.

Homing

Cornetz (**5, 6**) has recently published the results of an extended series of observations on the homing of five species of ants, four of which had poor powers of vision. His conclusions are as follows: Ants are not guided by a homing instinct; for, after a rain, ants carried a short distance from home cannot find the way back. Ants are neither led home mechanically by the odor of the outgoing trail, as claimed by Bethe; nor by a kinematic reverse of the movements of the outgoing ant, as held by Piéron, for the path of the incoming ant never coincides with that by which it left the nest. Ants have a vague notion of the distance traveled and of the angles made. A lone ant, returning to the nest from a distance, is guided by a memory clue, which was acquired during the outgoing passage of the ant; and an ant that has not traveled outward to the hunting ground cannot find its way quickly and directly to the next. Except the statement that the incoming trail never coincides with the outgoing, the above statements agree perfectly with a portion of the results published, five years ago, by the writer of this review in his dissertation on 'The Homing of Ants.' The numerous illustrations published by Cornetz show that the trails of the ants studied by him were not identical. In my work I have cited numerous instances where the incoming and the outgoing paths were different; but in many of our American ants, the difficulty of deciding whether the two trails are identical or slightly different is so great that I did not then, and do not now, feel justified in asserting that the outgoing and the incoming trails are never identical. The fact that they are frequently different justifies the second conclusion stated above. Cornetz asserts that light had no

effect on the ants studied by him; but it must be remembered that the visual organs of those ants were poorly developed. He further states that when a foraging ant starts home it moves, sinuously, in practically a direct line towards the nest. When near the nest, it makes a few exploring movements, which he has called a '*tournoiement de Turner*,' and then enters the nest.

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RECENT LITERATURE ON THE BEHAVIOR OF VERTEBRATES

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Fish.—Parker (13, 14) has been studying the sense of smell in the catfish and the killifish. He finds by tests on a freshly prepared fish head that currents pass into the anterior olfactory cavity and out of the posterior one in the case of the catfish, but that in the killifish, instead of such a continuous current, there is an intermittent one depending on respiration. Tests were made on the catfish with wads of cheesecloth, one containing minced earthworm, the other empty. The former was seized, the latter neglected. Fish with the olfactory tracts cut but the barblets, carrying the taste organs, normal, did not seize the food bag. Parker thinks this is evidence of a true sense of smell, a distance receptor. In the killifish, the empty bag was oftener seized, indicating that this fish uses the sense of sight more than the catfish; but operations on the killifish excluding the organ of smell did produce a loss of discrimination between full and empty packets, so that smell does play a certain rôle in this fish also.

Sheldon (16) reports that the dogfish can find hidden food, can discriminate between a packet containing a stone and one containing a crab, and fail to react to the presence of food if their nostrils are plugged. Like the killifish, they get a current of water through the olfactory openings by means of respiratory movements.

Bernoulli (2) has repeated Zenneck's experiments on hearing in fishes, testing trout and eels in open water by an electric bell, C3 in pitch, firmly fixed to its support, and rung under water. He got not the slightest evidence of reaction to the sound.

Parker (15) has published a study of the sense-organs of the dogfish. There is no skin sensitiveness to light in this animal. When the nictitating membranes are stitched together, so that the retinal images are abolished while awareness of light and darkness remains, the fish seek the light in accordance with their positive phototropism, but collide with objects in their path, showing that the retinal image is a guide in locomotion. The sensitiveness of the fish to the striking of a pendulum against the walls of the aquarium was very greatly reduced by cutting the eighth nerve, but was unaffected by cutting the optic nerve and by rendering the skin insensitive. The otoliths seem to be concerned rather with hearing than with equilibrium, as

removing them leaves the balancing powers unaffected. In opposition to Lee, Parker holds that the lateral-line organs are not specially concerned with equilibrium. Like the lateral-line organs, the ampullæ of Lorenzini are stimulated by pressure. The whole skin is sensitive to pressure.

Bauer's (1) method of testing the color sense in fish was to place the fish in a trough with blackened sides, the source of light being at one end, and diffuse light being admitted through the roof of the trough. The sources of colored light were sometimes cards and sometimes filters. The chief results of his experiments are as follows: (1) *Charax puntazzo* and *Atherina hepsetus* have an 'aversion' to the color red as a color. This is concluded from the observation that they swim away if anything red is brought near the open end of the trough, although the former fish is neither positive nor negative in its response to light, and both species will swim towards objects of any other color. (2) *Box salpa* seeks blue as a color: this is concluded from the fact that the fish, which is positively phototropic, prefers blue to white. (3) *Atherina hepsetus* and a species of Mugil show the Purkinje phenomenon, inasmuch as if a green is made lighter than a blue, so that the light-adapted fish seek the green, the dark-adapted fish will seek the blue. (4) Atherina's 'aversion' to red as a color is shown by the fact that if blue and red are so graded in intensity that the dark-adapted fish seeks the red, after light adaptation the fish goes to the blue even if it is very dark. Atherina is positive in its phototropism.

These experiments are rather severely criticized by Hess (10). He points out the inexactness involved in the varying methods of applying the color stimuli, and the apparent contradiction between the observation of the Purkinje phenomenon in Atherina and that of the result numbered (4) above: "the two cannot possibly both be true." So far as Bauer's alleged facts can be accepted, they can all, Hess thinks, be explained on the hypothesis that fish do not see colors as colors, but in the same brightness as a color-blind human being sees.

Amphibians and Reptiles.—Hess (11) has extended his researches on vision to amphibians and reptiles. His two principal methods are: (1) observing an animal's ability to pick up food illuminated by different intensities and qualities of light; (2) finding what intensity of white light is indistinguishable from a given color for animals that have a tendency to seek light. The present article summarizes his conclusions, not only for the two classes of animals just named, but

for all that he has tested. He finds that for fish, the relative brightness of different parts of the spectrum agrees with that for the color-blind or dark-adapted human eye; that for all other classes of vertebrates the spectrum has the same limits of visibility as for the light-adapted human eye; that the Purkinje phenomenon holds for amphibia, and that they seem to have the same color sense as man; "they behave in these experiments otherwise than as a color-blind human being would do." Further, reptiles and day-birds show a shortening of the spectrum at the blue end, probably due to the oil-globules. This shortening is greater in the turtle than in the fowl, and turtles have more orange globules. The pupillar reflex is present in birds but not in turtles. Adaptation phenomena occur even in rodless (turtle) retinas, and thus cannot be due to the rod-pigment. Apes have the same spectral range as man, and their faint light spectrum has the same brightness distribution. Probably their light and color sense agrees more or less closely with man's. This conclusion is of interest because it conflicts with the observations of Watson showing a low stimulating power of red on the monkey retina.

In Casteel's (4) study of the visual discriminations of *Chrysemys marginata*, the choice was offered between two boxes, food being given in one and an electric shock in the other. Four turtles learned to discriminate a black box from a white one, and one turtle failed to do so. None of the turtles learned to discriminate between two patterns, but these patterns were really so similar, owing to their being constructed so as to show an equal area of white, that the failure is not surprising. "Two turtles learned to discriminate between two series of parallel lines 8 mm. wide, one vertical and one horizontal in direction, and showed a fair degree of discrimination when these lines were reduced to 4 mm. in width. One of these turtles did well with a further reduction of the lines to a width of 2 mm." "Two turtles learned to discriminate between two series of parallel horizontal lines 8 mm. and 2 mm. in width respectively. One turtle learned first to discriminate between two series of parallel vertical lines 8 mm. and 1 mm. wide, next between lines 4 mm. and 1 mm. wide, then between lines 4 mm. and 2 mm. wide, and finally showed an excellent average of discrimination between lines 3 mm. and 2 mm. in width." About 183 trials were on the average needed to establish discrimination.

Cole (6) finds that brainless leopard frogs suspended with the hind legs dipped in solutions of ammonium and potassium chlorides

withdraw the legs more rapidly than when sodium and lithium chloride are used. The order corresponds to the degree of dissociation of the salts, which probably determines the time required for diffusion. A complicating factor is that of summation of stimuli. That the reactions are due to a chemical rather than to a pain sense is indicated by the fact that they persisted when ordinary pain reactions were abolished by cocaine.

Mast (12) gives a careful description of the mode of egg-laying in the loggerhead turtle.

Birds.—Herrick (8) has given an interesting account of field observations, made by means of mirrors, on the development of the young American black-billed cuckoo. He distinguishes three stages: the period of infancy, to five days after hatching, during which the grasping reflex, food reactions, the call note reaction, and the righting reaction are noted; the complete quill stage, about the sixth or seventh day, in which the birds sit up, notice sounds and visual stimuli more, peck at insects, preen themselves, and show fear; and a well-marked climbing stage, which lasts about fourteen days, during which the birds leave the nest and jump or climb about on the tree. The origin of the parasitism of the European cuckoo Herrick thinks due to "a change in the rhythms of the reproductive activities," the eggs being laid at more widely separated intervals than is the case with other birds. The American cuckoo shows a trace of this disturbance, but the difficulty of rearing the young thus produced has been met in the case of the American bird not by parasitism, but by the occurrence of the climbing stage, which enables the young to leave the nest at different times.

The same author (9) has published the first part of a study of the nest-building instinct, in which the most important feature is a classification of birds' nests on the basis of the type of behavior which they involve. Space does not permit giving the classification in full, but it is based on such principles as whether the bird has constructed the nest or adapted natural formations to the purpose of a nest; whether the nest is individual or coöperative, whether it is excavated or constructive; whether it is a standing nest or pendent.

L. W. Cole (5) has tested for chicks the law of Yerkes and Dodson that the optimal strength of stimulus for learning decreases the more difficult the discrimination required. The Yerkes brightness discrimination apparatus was used. Three differences in brightness were employed. For the medium difference the optimal strength of stimulus was less than for the greatest difference; and in the most difficult discrimination, while all the chicks learned under the weakest

stimulus used, two out of five failed with the strongest stimulus. So far, the law is confirmed; but the chicks which learned at all with the strongest stimulus learned more rapidly than they did with the weak stimulus. This unlikeness in the behavior of different chicks Cole thinks is explained by the fact that the chicks which failed were those which happened to make more wrong trials at the outset, and were discouraged from making further choices by the number of painful stimuli they received. The law does not, therefore, apply within the same limits as for mice, but it applies to a certain extent.

Watson's observations (19) on the homing of Tortugas terns were interfered with by the weather last summer. He got some instances of long-distance homing, however, and by plugging the nares found evidence that a nasal sense is not involved.

Mammals.—Bogardus and Henke (3) have found that white rats with the vibrissæ removed use nose and head contacts with the corners of a maze to guide them in the learning process, although no contacts need be made when the running has become automatic. Further, in learning a second maze altered in its middle portion from the first, the rats do better if the altered part is placed near the entrance; giving them a long familiar final part to traverse; they learn to make new short circuits more easily than to enter former cul-de-sacs; and they learn to take a new turn more easily the nearer to it the old path is blocked.

In the first section of Waugh's (20) paper on vision in the mouse five problems were investigated: discrimination of light intensity, color discrimination, perception of form, perception of the distance of an object from the animal, and perception of the third dimension of objects. Brightness discrimination was tested by boxes with black and white papers on them, light and dark violet also being used; and by the Yerkes apparatus for direct illumination. A novel feature of these tests lay in an attempt to show, by changing the backgrounds behind the boxes from light to dark, how far the discrimination previously established had taken account of the background. It was found that the background had had considerable influence. Color tests were made with Bradley orange-red and blue papers, between which the mice were able to distinguish; and with red, green, and blue filters. There was no ability to discriminate these colors: red of equal brightness with white to the human eye was discriminated from white, but the power to distinguish between them fell away as the brightness of the red increased, suggesting that the discrimination was based on brightness. Tests were made on brightness and color preference

by offering the mouse different yarns to use in making a nest. Black and white were preferred to grays, and red and yellow to blue and green. Albino mice did not discriminate brightness so well as the others, nor did they distinguish red from white light. Tests with two cards having differently shaped openings showed little power to discriminate form. Distance perception was studied by placing the animals on a small disk which could be raised to different heights above a bench. The fact that the mice jumped more readily from the lower heights was taken as evidence that vision functions in the estimation of distance under these conditions. Another test was made by means of a box with two adjustable partitions, each extending half way across the width of the box, from opposite sides, in such a way that if one partition was nearer the end than the other a passage was left between them. If the right hand partition was nearer, escape could be had by turning to the right around the end of this partition: if the left hand partition was nearer, the turning had to be made from the left. The mice proved unable to judge from the end of the box which partition was nearer and govern their turns accordingly. It may be noted in criticism that this test really demanded not merely distance perception, but a very high type of learning which would hardly be expected of the mouse.

That a small field of binocular vision exists in the mouse was concluded from observing that the two eyes of the animal can both be seen at once from a certain range of positions. The mouse seems not to make eye movements for fixation and convergence, and a microscopical study of the retina revealed no fovea. All parts of the retina are probably equally sensitive. No cones were found.

Shepherd (17) seems to have established the fact that raccoons can be taught to discriminate between the sound of their own individual names and other articulate sounds.

Hamilton's (7) Study of Trial and Error Reactions reports a novel type of test. The subjects were a normal man, seven children ranging in age from fifteen years to twenty-six months, a defective man and a defective boy, five monkeys, sixteen dogs, seven cats, and a horse. The apparatus was as follows: a room was constructed from which exit could be had from one of four doors, and the particular door which could be opened was changed from experiment to experiment. The types of behavior displayed by his subjects Hamilton classifies as follows. In Type A, the recation includes a single definite effort to open each of the three inferentially possible doors, and does not include an effort to open the inferentially impossible door, *i. e.*, the one which was the exit door in the preceding test. Behavior of

this type means awareness of the impossible door as such. It was found in all the human subjects above eight years of age except the defective man. Type B involves trying all four doors, once each, in irregular order. This type was found with diminishing frequency in the defective man, the monkeys, dogs, cats, and least in the horse. Type C involves opening each door once, in a definite order, beginning with one end of the row. This is especially apparent in the young monkeys and a defective boy, although it is also shown to a considerable extent by the dogs. Type D "involves the error of making more than one . . . effort to open a given door during the same trial," but always with an interval of attempts to open the other doors. This recurrent tendency to try a particular door reaches its maximum in the mature dogs, and decreases lower down in the animal scale. The lowest type of behavior, E, involves repeated efforts to open the same door or group of doors without trying others in the interval; or continued avoidance of one door. It increases in frequency as we pass from higher to lower and from older to younger animals; it is almost wholly lacking in the mature human beings, even the defectives, but is frequent in the infant.

The most important results of Shepherd's (**18**) work on monkeys may perhaps be summarized under three heads: those on color vision, on auditory discrimination, and on ideas. The method of testing color vision consisted in offering the animal a choice between bits of rice dyed different colors, the pieces of a certain color having been soaked in quinine. Color vision is inferred because the monkeys that had learned to avoid red rice in red-white tests avoided pink rice in pink-green tests. To suppose that owing to the brightness difference between red and pink to the human eye this behavior could not have been based merely on brightness discrimination is to neglect the possibility that the brightness value even of pink may be less than that of green to the monkey eye. The monkeys proved able to discriminate a considerable difference in the loudness of two noises and a difference of two octaves in pitch: no tests were made with smaller pitch differences. Some evidence of inferential imitation was obtained. The author thinks that the presence of ideas, "in the sense of an understanding of a certain relation or situation," is demonstrated by such observations as the following: a long board was placed with one end in the cage and food on the other end, which projected out from the cage beyond the monkey's reach; the animal after pulling at the wire netting of the cage a few moments seized the end of the board and pulled it into the cage. It hardly seems necessary to see in this behavior anything more than the monkey's

racial habits. The board is as it were the nearest portion of the attractive object, and he pulls as his ancestors have pulled at a branch with a nut at the end. It may well be that monkeys have ideas, but this particular piece of behavior hardly indicates the fact any more than do other adaptive but inherited tendencies to movement on the part of animals much lower in the scale.

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SPECIAL REVIEWS

RESPONSES TO LIGHT AND COLOR

Light and the Behavior of Lower Organisms. S. O. Mast. New York: John Wiley & Sons, 1911. Pp. xi + 410.

Two years ago, Miss Washburn, in *The Animal Mind*, gave us our first critical and historical survey of that body of facts which may best be called animal behavior. Her presentation, covering as it did a multitude of facts, could not treat of any one topic in an exhaustive way. The present volume by Mast treats of a limittd portion of the field of behavior—that of orientation to light. When one considers, however, that Mast's bibliography consists of 13 pages of 8 point type, one cannot but feel that if the author is treating of a 'limited' subject, it is at least one which has been studied in a very broad way. As a matter of fact, Mast deals in the book historically and experimentally with those vexed questions of the mechanism of light-orientation (tropism, *Unterschiedsempfindlichkeit*, etc.), the variability and modifiability of such responses, and the adaptiveness or lack of adaptiveness of such responses. The student of behavior who does not find his especial province in the field of lower organisms, and who yet tries to follow this work, knows what an almost hopeless task it is, due to the general complexity of the subject, to the lack of a definite and uniform vocabulary, and to the spirit of strife which so often seems to be the initial impulse to experimentation in this field. Mast's book attempts to deal in a calm and dispassionate way with the actual facts in the case. He lets his controversial spirit appear somewhat, when he discusses Loeb's work. With this exception the book is well balanced.

The volume is divided into four parts: Part I. deals with the historical side of plant and animal orientation. On pages 53 ff. Mast gives some fifteen definitions of tropism: "it is evident that nearly every reaction in any living organism comes under one or another of the various definitions given to the term tropism." So conflicting are the uses of the term that Mast feels that it has outgrown its usefulness, and accordingly avoids it in the book. He uses 'orientation to light' instead of phototropism, etc. Under the general headings of orientation the following pertinent questions may be

asked,—all answerable by experiment: Is orientation direct; does the organism turn directly away from or toward the source of stimulation, or does it become oriented after a series of preliminary movements? How is the stimulus causing orientation produced? By direction of rays through the organism, in accord with the theory of Sachs; by absolute difference of intensity on symmetrically located points on the sensitive surface, in accord with the theories of Loeb and Verworn; or by changes in intensity on the surface in accord with the ideas of Engelmann, Darwin and Jennings? Furthermore, after the animal has become oriented, does light act constantly as a directive stimulus, or only when the organism turns out of its course, thereby producing changes in intensity? Finally, is orientation due to the direct effect of light upon the locomotor appendages (Verworn, Torrey), or to the indirect effect through a reflex arc (Loeb), or finally, is the whole organism involved in the process (Jennings)?

These in part are the questions which Mast sets out to answer, both by consideration of the works of others, and from his own experiments. The author proposes in his experiments to enter very much more deeply into the subject of light responses than was done in the superficial work of Loeb and his followers.

Part II. concerns itself with a detailed experimental answer to the questions just raised. The process involved in plant orientation to light is first discussed. Mast finds in the behavior of plants to light no support to Sachs's ray-direction theory, nor to that phase of Loeb's theory which asserts that symmetrically located points on the surface must be struck by light at the same angle when the plant is oriented. *Orientation is regulated by differences in light intensity on opposite sides of the plumules.*

Then follow the details of orientation in unicellular forms, such as amœba, euglena, stentor, paramecium; in colonial forms as volvox; in cœlenterates, as hydra and medusæ; in vermes as the earthworm and planaria, in fly larvæ and in the echinoderms. Still higher forms are studied, such as arthropods and vertebrates (frogs and toads). While the author is dependent to some extent upon the work of others as to his conclusions, in general he has worked very carefully and thoroughly with most of the forms discussed.

He finds that orientation in such forms as the amœba is due to local response to a local stimulation. Light retards the activity of the protoplasm and thus prevents the formation of pseudopods upon the more highly illuminated area. In forms like euglena, stentor (and probably all the ciliates and flagellates which orient

in light) orientation is due to definite responses to changes of light intensity on the sensitive part of the organism. The rotation upon the long axis of the body brings about, by shading the sensitive area successively, now high, now low intensity. As soon as the animal reaches a position in which the rotation on the long axis no longer causes changes in intensity on the sensitive region, there is no longer cause for turning. Similar factors are involved in the responses of colonial forms. Representatives of the cœlenterates (*e. g.*, hydra), vermes (earthworm, planaria, etc.), mollusks, arthropods and vertebrates, orient directly. In most cases there are a few trial movements. From all this work Mast concludes that orientation may be due (1) to local response to local stimulation (amœba); (2) to shock movement, avoiding reactions as in ciliates, etc.; (3) to differential response to localized stimulation, as in cœlenterates, vermes and the higher forms; (4) to sight proper, as in animals with image-forming eyes.

There is no evidence anywhere in support of Sachs's ray-direction theory, nor of that hypothesis which asserts that symmetrical points must be struck by light at the same angle. Nor is there any conclusive evidence except in the case of animals with image-forming eyes that light acts continuously as a directive stimulus. There is no conclusive evidence that orientation to light is ever due, in any organisms, to tropic reactions, if the definitions of tropism by Loeb, Verworn and Rádl are used as criteria.

All organisms that respond to light respond to changes in intensity. In some cases orientation follows upon these responses; in others it does not. All light response in such organisms is *unterschiedsempfindlich.* They all respond to time-rate of change, as explained by Engelmann, Bert, and Graber, years ago, and more recently by Loeb.

Part III. is given over to a general consideration of responses to light, the adaptiveness or lack of adaptiveness in such reactions, regulation of reactions, variability and modifiability of reactions, etc.

Part IV. deals with reactions to monochromatic light. In this latter part the reviewer would take issue with Professor Mast in regard to the adequacy of the methods used in the study of orientation to monochromatic light. Mast feels that the methods have been adequate; but surely there has been little effert on the part of these investigators to obtain anything like adequate control of stimulation.

The book as a whole gives us another landmark. The author

has been sincere and earnest in his work, conservative in his conclusions and generally temperate in his criticisms.

JOHN B. WATSON

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The Instinct of Self-concealment and the Choice of Colors in the Crustacea. ROMAULD MINKIEWICZ. Annual Report of the Smithsonian Institution, 1910, pp. 465–485. Translated from Rev. gén. d. sci. pures et ap. Paris, Feb. 15, 1909.

The author brings together here in a general way all of the results of experiments described in a series of seven or eight papers, some originally published in Polish, others in French. Nearly all of these papers have been reviewed in this journal in past years. I wish however to state again a point emphasized in these reviews, *i. e.*, that Minkiewicz maintains that crustacea select and cover themselves with fragments of various kinds but of such a color as to harmonize with their environment. This is in opposition to the results of all other investigators who have made observations along this line. There is consequently much doubt in the minds of some concerning the accuracy of Minkiewicz's observations. In view of this fact and the importance of the subject more experimental observations are highly desirable, especially on the species used by Minkiewicz.

S. O. MAST

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Certain Habits, Particularly Light Reactions, of a Littoral Aradnead. T. H. MONTGOMERY, Jr. Biol. Bull., **20**, 71–76.

This little beach spider, when shaken from eel-grass under which it is usually found, was observed to run persistently landward except when the sun is directly above or when it is obscured. The author found that these animals in the laboratory react negatively to lamp light and sun light, and after demonstrating that their course landward is not the result of responses to moisture, wind or gravity, he concluded that it is due to a negative reaction to light reflected from the water.

It seems to the reviewer however that if this were the only factor directing their course one would expect them to go landward at noon as well as at other times, for surely the differences between the amount of light reflected from land and water at that time is as great as it is at some other times, *e. g.*, when the sun is on the landward side of the beach and its rays strike the water at a rather small angle.

S. O. MAST

JOHNS HOPKINS UNIVERSITY

MEMORY

L'évolution de la mémoire. HENRI PIÉRON. Paris: Flammarion (Bibliothèque de philosophie scientifique), 1910.

It is now more than forty years since Professor Ewald Hering delivered his famous lecture on 'Memory Considered as a General Function of Organic Matter.' Itself clear, brilliant and masterly, that lecture has, nevertheless, more than once offered others occasion for extravagant and confused thinking. Setting out from the principle of psychophysical parallelism, when scientific psychology was in its first beginnings, Hering made it evident that the mnemic basis of reproduction lies in neural 'retention,' and that the property of organic conservation is inherent not only in the human cerebral cortex but in living matter at large. There appeared in the lecture no obfuscation of fact by speculation, no attempt to make the writer's *Fach* a *Grundwissenschaft*, no confusion of brain and mind, and no unwarranted inference out of one science into another. Not so much can be said for most of Hering's imitators; not so much can be said for Piéron's *Evolution of Memory*. Piéron is writing a psychological treatise; yet he begins by repudiating introspection, by confusing the memorial, retrospective consciousness with persistent effects of organic function, and he proceeds by searching the field for an 'objective psychology,'—very much as Lewis Carroll's Bruno may have looked for 'black light,' or as the restless inventors of Balnibarbi tried to fashion sofa-pillows out of marble.

In conscious opposition to his countryman Bohn, the author views the racial development of mind as a continuous process. So far as the function of memory is concerned, he succeeds; but he succeeds only by the help of a definition so broad—*all those facts of behavior that point to antecedent events in the individual's career*—as to rob the term of its psychological significance, and by the disregard of essential differences to be found among the processes of adjustment throughout the animal series.

No general characteristic of mind stands out so plainly from the experimental work of the last ten years as the fact of *determination.* Consciousness flows. Its height, volume, direction and accomplishment depend quite as much upon its head and the tributaries leading down out of the past, as upon its temporary composition and its immediate organic setting. Thinking is carried out under *Aufgabe;* action is dependent upon determining tendency, habituation and practice involve progressive elimination and selection, and emotion runs its course under the pressure of whole generations of affective

experience. The psychophysical organism is well-nigh unlimited in its varied resources for bringing into play the residues of past function. Impressional and reproductive tendencies lie dormant for hours, days, or even years; the brain is shot through with predispositions and sets; the movement-apparatus tingles with prepared coördinations; instructions and suggestions echo and re-echo; and the bed of consciousness is cut deep in the organic foundations of the species. To throw all these together—physical and mental—and to account them as 'memories' is only to confound that which has taken years to distinguish and to set in order.

The sections of the book on the persistence of rhythms beyond the term of the provoking stimulus survey a field where the author has himself done valuable work at first-hand. The core of the book is the part devoted to learning and habituation among animals. It brings together in a helpful way most of the recent literature of behavior and of comparative psychology.

MADISON BENTLEY

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ANIMAL PSYCHOLOGY

La nouvelle psychologie animale. GEORGES BOHN. Paris: Alcan, 1911. Pp. 200.

We have here a clear and succinct statement of Bohn's attitude with regard to the problems of comparative psychology. The book is divided into three parts, the first dealing with the lower invertebrates, the second with the arthropods, and the third with the vertebrates. The behavior of the first group of animals Bohn thinks may be most profitably studied from the physico-chemical point of view taken by Loeb. It reveals three principal features: tropisms, differential sensibility, and cellular memory. A tropism is a reaction determined by the relative speed of chemical changes produced in symmetrically placed elements on the two sides of an animal's body. The notion of symmetrical structure is essential to the tropism. In the case of differential sensibility we have a pause, a recoil, or a rotation through 180° on the part of an animal affected by a sudden change in the intensity of a stimulus. The rudiments of associative memory are also found in the lower invertebrates. These three factors in behavior are modified by the physiological state of the animal, which in turn depends on the individual's own activities, on its habits of life and individual diathesis, on changes in the environment, and on habitat. The idea of finality should be practically

abandoned in the study of the behavior of the lowest animals; tropisms are often fatal to the animal, although differential sensibility shows more adaptation to the needs of the organism.

The problem of instinct is the chief problem encountered in the study of the behavior of higher vertebrates. Instincts according to Bohn are "complexes of activities into which various elements enter: inherited elements and those due to individual learning; relatively simple elements (tropisms, differential sensibility), and more complicated factors (associations of sensations)." He analyzes the following cases of instinct in arthropods: death-feigning, nest-finding, food-seeking, mimicry, and social instincts.

The development of the brain in vertebrates brings with it an extensive development of the memory factor in behavior. "Intelligence is the appanage of vertebrates." In the discussion of the methods which have been used to investigate the behavior of vertebrates the largest amount of space is devoted to Pawlow's method. The other methods are classified under the terms labyrinth method, puzzle-box method, method of imitation, and method of training (*dressage*); the last two are held to be of distinctly less value than the others.

M. F. W.

BOOKS RECEIVED DURING JULY

GROOS, K. *Das Seelenleben des Kindes. Ausgewählte Vorlesungen.* Berlin: Reuther & Reichard, 1911. Pp. 334. Mk. 4.80.

BROWN, W. *The Essentials of Mental Measurement.* Cambridge: Univ. Press, 1911. Pp. vi + 154. 3/6 net.

OFFNER, M. *Das Gedächtnis. Die Ergebnisse der experimentellen Psychologie und ihre Anwendung in Unterricht und Erziehung.* Berlin: Reuther & Reichard, 1911. Pp x + 258. Mk. 3 50.

DESSOIR, M. *Abriss einer Geschichte der Psychologie.* Heidelberg: Winter, 1911. Pp. xi + 272.

MILLE, P. *Caillou et Tili. Psychologie de l'Enfance.* Paris: Calmann-Lévy, 1911. Pp. 269. 3 fr. 50.

KLEMM, O. *Geschichte der Psychologie.* Leipzig und Berlin: Teubner, 1911. Pp. x + 388. Mk. 8.

SINCLAIR, S. B., and TRACY, F. *Introductory Educational Psychology. A Book for Teachers in Training.* Toronto: The Macmillan Company of Canada, 1909. Pp. xii + 180. $0.90 net.

MCKEEVER, W. A. *Psychologic Method in Teaching.* Chicago: A. Flanagan Co., 1909. Pp. xxi + 332.

NOTES AND NEWS

Dr. Knight Dunlap, of Johns Hopkins University, has been advanced from an associateship to an associate professorship in psychology.

Professor J. H. Leuba, of Bryn Mawr College, recently received the honorary degree of Doctor of Science from Hobart College.

It is perhaps not generally known that the Sarah Berliner Research Fellowship for Women (an annual fellowship of $1,000 "open to women holding the degree of doctor of philosophy or to those similarly equipped" and "available for study and research in physics, chemistry or biology") and the biennial prize of $1,000 offered by the Naples Table Association ("for the best thesis written by a woman, on a scientific subject, embodying new observations and new conclusions based on an independent laboratory research in biological, chemical, or physical science") are open to workers in psychology. Applications for the exact conditions should be made to Mrs. C. L. Franklin, chairman of the Sarah Berliner committee, 527 Cathedral Parkway, New York, or to Dr. Jane Welch, Baltimore, Md., of the Naples Table Association.

The present number of the BULLETIN, dealing especially with comparative psychology, has been prepared under the editorial care of Professor Margaret Floy Washburn.

ERRATA.—Through an unfortunate mishap in the printing office several sentences of our July issue were so mutilated as to render them almost unintelligible. The lines which suffered (page 246, lines 21–26) should read as follows: . . . . . compend for readers and investigators of this topic.

In Langfeld's study of association (**24**), pictures of familiar objects served as stimuli; and the instructions to the reagents differed from those ordinarily given in that the response must not contain the name of the pictured object. Langfeld found that negative instructions are effective in determining associations,—*i. e.*, the *Aufgabe* . . . . . It is but fair to Dr. Baird to state that none of these misprints appeared upon the author's proof submitted to him, and that he is in no way responsible for the errors which appeared.

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THE

PSYCHOLOGICAL BULLETIN

GENERAL REVIEWS AND SUMMARIES

MEMORY, THOUGHT, JUDGMENT, LOGIC (THEORY)

BY PROFESSOR W. H. SHELDON

Dartmouth College

Out of twenty-six French or German books or articles examined for the present report, but five seemed clearly relevant, while yet remaining in the non-experimental field. These are concerned with alleged new mental states such as 'Annahmen,' 'Objektive,' 'Gestaltqualitäten,' and allied forms. This indeed is to be expected, as the older known states, judgment, conception, idea, etc., are now being submitted to experimental investigation. No less a thinker than Meinong suggests (10, Preface, p. xii) that the newly discovered processes should now be so tested. Most important among the theoretical discussions seems to the reviewer to be the second edition of *Ueber Annahmen* (10). It is extensively recast, containing replies to the criticisms and interpretations of Marty, Russell, Kerler, and others, besides some new argumentative material—in all, ninety-seven pages more than the first edition had. The distinction between 'Annahmen' and 'Vorstellungen' is more thoroughly defended, the 'Objektive' are assigned a more fundamental position, and many questions of detail are more exhaustively treated. According to Meinong, 'Objektive' are apprehended by 'Annahmen,' but the latter are by no means, as Russell claimed, ideas of objectives. The crucial reason why no 'Annahmen' can be ideas is that there are negative 'Annahmen,' whereas no idea can be negative; for negation has no degree (10, p. 14) while ideas may have. Much metaphysics is scattered through the book; of greatest interest to the psychologist are perhaps Chapter III., which is a new chapter on 'Das Objektiv,' and Chapter X., entitled 'Ergebnisse. Bausteine zu einer Psychologie

der Annahmen.' In Chapter III. the 'Objektiv' is defined as the 'Gegenstand' of a judgment (pp. 43, 44) which subsists ('besteht') but does not exist. Further, wherever there is an 'Annahme' there is an 'Objektiv' (p. 131). Yet the two must not be identified (p. 143). An interesting psychological definition of the 'Annahme' in Chapter X. calls it a limiting case of the judgment, with belief at the zero-point (p. 344). 'Annahmen' have degree of evidence-from-other-'Annahmen' (as in hypothetical inferences) and thus have an analogy to belief. For the rest, the book deserves a detailed exposition, from the psychological point of view, in English. Probably few American psychologists are acquainted with the very suggestive work of the Austrian professor, and in the German it is exceedingly difficult and involved.

Kerler's criticism of Meinong (6) would reduce the 'Annahmen,' even as Marty's criticism some years ago would do, in most cases to ideas. Kerler claims that ideas *can* be negative (p. 7ff.). For since all objects may have ideas corresponding to them, quite as well as judgments about them, there may be negative ideas as easily as negative judgments. The play-world, an important source of supply for 'Annahmen,' contains only ideas. The actor *thinks of* the character he represents, but does not suppose himself to be that character. In fiction or poetry all that is needed is 'dass dem Dichter die Charaktere in voller Klarheit und Anschaulichkeit vor der Seele stehen' (p. 14). Lies are only ideas, for the liar does not suppose the situation he describes to be true. Scientific hypotheses made for *reductio ad absurda*, questions, fairy tales, antecedents in hypothetical judgments, are in turn disposed of as ideas. Nothing has shown more clearly than this criticism and Meinong's replies to it, the need of experimental study in the field of 'Annahmen.'

A paper by A. Gelb (4) on 'Gestaltqualitäten' attempts rather to clear up the general situation than to put forth new facts. He questions whether the 'Gestaltqualitäten' are anything more than sums of the relations in a complex. Yet if they are no more the unity is lost among the parts. The required element cannot properly be sought in the combining activity of the mind, for relations and their unity are as much 'given' as are terms (p. 45). The author is of the opinion that whatever explanation can be given must come from the observed character of the relations and their observed mode of combination (p. 57). Professor Groos, writing on the allied topic of the formation of thought-systems, particularly in regard to the relation of opposition, finds that the latter originates in a 'Stauung'

(5, pp. 181–2) which is extra-intellectual; in other words, sudden changes, breaking in upon sameness, surprise us. Adding to this the judging process, we have the origin of contradictory propositions. All springs from the biological dualism of expansion and contraction (p. 196). Next we have a paper by E. Claparède (2) which turns to the general topic of imageless thought. His position seems based upon philosophical criticism. "L'image est un phénomène commode pour la description, . . . statique et inerte, n'est pas du tout propre à rendre compte de ce qui fait le caractère de la vie mentale, à savoir l'adaptation" (p. 375). One cannot invoke images to explain understanding, for then the image itself could be understood only by another image. The general position of the paper is functional. Quite different is Piat's treatment (11) of the concept.[1] The concept is something definite and real, not an abstraction; yet it is not simply a derivative of sense. It is to be explained by a non-sensational, spiritual activity. The paper contains an historical account of views of association and thought.

Of the publications in English, most are built upon philosophical or logical bases and contribute little of the psychological. The following, however, contain psychological material. Pillsbury's *Psychology of Reasoning* (12) is an analysis of belief, conception, judgment, inference, and proof, along traditional lines. The book does not claim originality, but furnishes rather a summary of current tendencies for those interested in the thought-processes. A very complete history of views in regard to the psychological nature of belief is found in the article by J. Lindsay (7). He summarizes the views of Hume, Fichte, Fries, Brentano, Bain, Mill, Beneke, Spencer, Sully, Baldwin, Stout, Lipps, Dewey, Schultz, James, Paulsen, Royce, Sigwart, Windelband, Rickert, Heymans and Wundt. His conclusion is: "Belief, it will be seen, is best taken, for psychological purposes, not as mere reality-feeling or mere implicit belief, but as the assertion of a reflective judgment or determination. . . . In the case of a thing, belief consists in the appearance of reality, in the case of a proposition, belief lies in the appearance of truth which it carries for the mind" (pp. 305–306). In favor of the presence of 'theoretical' interests in the psychological beginnings of thought we quote from Professor Creighton's paper (3): "If the genetic viewpoint is to be retained, it is necessary to maintain that the cognitive mind was never *merely* practical, but that, even in its first beginnings,

[1] For summaries of this, and of the publications following it in this review, the writer is indebted to the kindness of Dr. Felix Arnold.

logical functions and logical meanings were not entirely lacking, though for the time overshadowed by more pressing interests" (p. 62). The paper contains a criticism of the point of view of Baldwin's genetic logic. Professor McGilvary (8) studies the psychological basis of the logical processes. He finds that the meaning of an idea is the 'fringe.' The object to which the idea refers is a something, missing in the experience of the idea-content, to which that fringe points. An interpretation of the consciousness of meaning from the social point of view is furnished by the paper of Professor Mead (9). "The consciousness of meaning . . . is a consciousness of one's own attitudes of response as they answer to, control, and interpret the gestures of others" (p. 405). Such consciousness implies (1) a social situation or stimulation by acts of others, (2) consciousness of the value of one's own gestures in terms of the change in gestures of others, and (3) the feel of one's own attitude arising to meet the gestures of others. Finally we have the paper of Professor Calkins (1) which takes issue with the sensationalism of Titchener. She defends a relational view of the thought-elements, and criticises the interpretation of meaning as association or context. Her position is akin to that of James and Woodworth.

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THE PSYCHOLOGY OF TESTIMONY

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Relatively only a small amount of experimentation has been conducted during the past year upon the psychology of testimony. Binet (2), indeed, who deserves credit for initiating the work in this field, speaks as if the earlier investigators had garnered substantially all the really valuable fruits of experimentation, and as if there remained but sparse picking for the investigator of today. But how can this be so, we are tempted to inquire, when the problem of testimony in its relation to historical investigation has hardly been touched upon and when the problem of adapting the results of the experimental study of testimony to the practical needs of jurisprudence still calls for solution?

We may note in the recent literature signs that the legal profession is showing greater readiness to consider the results of experimental psychology. Thus, Gmelin (5) has presented in an interesting manner the points of contact between the psychology of testimony and legal practice and has urged the prime need that every jurist should familiarize himself with the work of the laboratory. Duprée (3) has published in a popular form an extremely good summary of the net results of the laboratory studies of report, while Gross (7), the eminent criminologist and jurist, declares that all testimony, from whatever source, ought to be investigated critically with the aid of the doctrines of 'subjective criminal psychology,' and suggests that experimentation may some day enable us to classify witnesses into groups (by age, sex, training, temperament, etc.) and to state for what sort of observation and report each of these groups presents peculiar gifts or defects, so that, in a specific case, given the type of witness and the type of event to be reported, we may be able to predict whether the witness can tell the truth, even if he wants to.

Aside from these general discussions of the relation of the psychology of testimony to law, we may note in the recent literature four chief queries: First, how important for practical application are the conclusions drawn from the standard picture-tests? Second, is the testimony of children as unreliable as has been claimed? Third, can the religious oath be set aside without appreciable loss of accuracy in testimony? Fourth, how great and of what order is the unreliability of the testimony of mental defectives?

Gross seriously questions the importance for law of the 'discovery' in the laboratory of the possibility of classifying witnesses, by means of the picture-test, into those graphically (pictorially) minded and those not graphically minded. We can not, he says, argue from the outcome of this test to the grade of general intelligence of the witness, nor can we argue his ability to report correctly events that transpire in temporal sequence. For these reasons, Gross urges the extended use of the event-test in place of the picture-test, as being quite as simple to arrange, much more significant for the measuring of observation and report, and closer to the actual situation of the witness in court. Gerland (4), however, believes that the picture-test is significant for legal procedure, and that the test can be profitably carried out in court, both to test special ability to depict a scene and to test general ability to report.

Dr. Baginsky (1), the expert German specialist in children's diseases, declares that children are the most dangerous of all witnesses and demands that their testimony be excluded from court record wherever possible. Similar statements are made by Duprée. Gross, however, stakes his thirty years of experience in the court against the views of these physicians. He declares that a healthy half-grown boy is the best possible witness for simple events. He thinks that Baginsky may have been influenced by his professional contact with sick children, but that, more likely, he, like many another, has been led to generalize too hastily from a few instances of inaccuracy in children's testimony and without stopping to consider the equally numerous inaccuracies in the testimony of adults. Children, according to Gross, make different errors but no worse ones than do adults. In some respects, *e. g.*, freedom from prejudice, erroneous interpretation, emotion, intoxication, etc., a child is better fitted than an adult to give accurate report. The present writer would suggest that the whole matter could be very simply cleared up by an appropriate experiment. Why not subject observers of different ages to a graded series of event-tests?

Duprée, briefly, and Gmelin, at length, inveigh against the use of the religious oath, on the ground that the oath is a relic of primitive conditions, and contrary to the spirit of modern civilization, that it neither increases the veracity of the honest, nor keeps liars from lying, nor lends intelligence to the mentally defective, and that its abolishment would make one more desirable step in the separation of church and state. A simple affirmation of the witness, supported by the control that would be exerted by a plain statement from the

judge as to the punishment of perjury, would be just as effective in securing reliable testimony as the present religious oath.

A general account of the faulty testimony of defectives (the feeble-minded, epileptic, hysteric and insane) is contributed by Duprée, who warns emphatically of the peculiar danger that exists when the defect is latent or mild and unsuspected by the court. Especially interesting are cases of morbid confessions, and self-accusation (with or without the accusation of other innocent parties). Gregor (6) studied experimentally, by means of the picture-test, the reports of normal and of abnormal subjects under varying conditions of exposure and time-interval. Patients afflicted with various paralyses proved to be not appreciably poorer than normal subjects (nurses) when the conditions were favorable to report, but their reliability and their range of report fell off markedly and their resistance to suggestive questions was reduced when the conditions were unfavorable for report (long time-interval, interrogatory, etc.). Their defectiveness, in other words, is not so much in observation, as in the recall and critical organization and formulation of what had been observed.

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SUGGESTION

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Starch (8) tested the suggestibility of students by measuring the extent to which they changed their handwriting in copying material, (*a*) printed, (*b*) vertical script, (*c*) slanting script, (*d*) unusual large script. The 'unconscious' imitation was quite general and astonishingly great. Similar but less striking results would have been secured if the students' attention had not been called to the subject of hand-

writing by the directions which were given them: "We desire records of your handwriting. Will you accordingly write out the words and sentences presented on the pages given you?"

Cohn and Dieffenbacher (2) made what appears to be a very thorough use of suggestive questions in testing the suggestibility of children—ages 7–20. The children were shown pictures, after which they were asked a series of well formulated questions concerning the details of the pictures. Some of the questions were suggestive questions. As a result of their tests the experimenters assert: (*a*) that the suggestibility of boys and of girls is practically equal, but that the boys are slightly more inclined to take the critical attitude; (*b*) that suggestibility decreases with age, this tendency being more marked with girls than with boys; (*c*) that bright boys are as suggestible as dull ones, but that dull girls are more suggestible than bright ones. The experimenters seem unaware of the fact that the apparently greater suggestibility of the younger children may be merely a greater docility and a greater desire to please by affirming the suggested answers.

Hartenberg (4) contends that there are two forms of suggestibility: (*a*) lack of criticism, liability to neurasthenia, passive; (*b*) excessive impulsiveness of ideas, liability to hallucinations and automatisms, liability to hysteria, active. There is certainly just ground for such a classification and it may be found of distinct value in classifying individuals.

Elst-Gaume (3) also attempts a twofold classification of suggestion: (*a*) morbid, unconscious, infallible, sudden, passive; (*b*) normal, conscious, problematic, active. The reviewer is unable to see the wisdom of this classification.

Bajenoff and Ossipoff (1) argue that suggestibility is greater in the waking state than in hypnosis. Their data do not substantiate such a thesis. They do however draw attention to the fact that suggestion is less powerful in hypnosis than is assumed by the laity to be the case.

Scott (7) attempts to present the psychology of suggestion; to show the extent to which it is utilized in influencing men; and to indicate methods of utilizing it wisely in commerce and industry.

By means of narrations of personal experience as a teacher and by numerous citations from psychological and pedagogical literature Plecher (6) emphasizes the importance of suggestion in the life of the child and the necessity, therefore, of giving the child the most helpful suggestions.

As a result of extensive experiments made with subjects in a waking condition Jung (5) finds a high degree of suggestibility for all subjects. He verifies the results of many other experimenters as to the greater suggestibility of women as compared with men, and as to the greater suggestibility of the young as compared with the older.

Thiele (9) made a study of persons whose eye troubles were diagnosed as due to suggestion and in no way to be dependent upon organic difficulty. The following conditions were discovered but were not at all common: they desired to wear glasses; they manifested other hysterical symptoms; their families were neurotic; they were in bad health and undeveloped.

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GRAPHIC FUNCTIONS

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The literature of the past year shows an increasing interest in writing and drawing activities. This interest is practical rather than theoretical and is centered in questions concerning the pedagogy of drawing and writing, and the utilization of handwriting in a study of character or for evidential purposes.

A most valuable contribution to the latter aspect of the subject

is Osborn's recent book (8), which has been highly commended by Jastrow (3) for its many excellent qualities. While Osborn's volume is in no sense a contribution to the psychology of handwriting, it should serve such an investigation by pointing out the technique used in identification of handwriting by practical experts who with great ingenuity utilize the most modern of mechanical appliances such as the document microscope, enlarged photographs, appliances for accurate measurement of the line-stroke, writing-slant and the like. Osborn's account of the different handwriting systems and of the dependence of handwriting appearance upon writing-materials will also be found helpful. As Jastrow (3) suggests, the refinement of the methods of studying handwriting may in time justify its use for diagnostic purposes.

Schneickert (11), also interested in handwriting from the evidential point of view, has occupied himself with the technique of classifying and indexing collections of handwriting specimens obtained from criminals. The card-catalog system invented by this author may be of value to the general student by suggesting a practical system of docketing. Moreover, the testing in practical service of those details which Schneickert has chosen as the basis of his classification must contribute somewhat to our knowledge of the fundamental graphic characteristics, that is, those least subject to voluntary disguise. Such a testing of Schneickert's system of classification is assured by the fact that it has already been adopted by the police authorities of Berlin and other German cities.

That the interest in graphology maintains its hold in Germany is shown by the appearance of a scholarly work (4) by Dr. Klages of the German Graphological Society. Klages retains the name graphology in the title of his work in order to mark certain affiliations but describes his theme as the science of expression. An estimate of Dr. Klages' volume would involve a comparison of it with the standard books on the subject and will, therefore, demand a separate review.

In great contrast to such a volume is the little primer by Booth (1), which summarizes popularly and non-critically some of the conventional beliefs of current graphology,—a book which is of interest in this connection only as marking the meagre interest in America in the general subject, in contrast to the time and thought devoted to the topic in Germany and France.

The investigation of family resemblances in handwriting, the study of character as revealed by handwriting and of the general effect of education and social environment upon handwriting-appearance, have

each raised the question as to the extent to which handwriting may be modified by unconscious imitation. Starch has broken the ground for such a determination in his work on 'Unconscious Imitation in Handwriting' (12). Starch found that both the slant and the size of letters were influenced by the copy present. Starch's women subjects showed a greater tendency toward imitation than did his men subjects, an interesting point in connection with the general fact that the handwriting of women is less original, that is, follows social models more closely, than does masculine writing. Starch found little evidence of a negative change, although on general grounds this might have been expected, since what Royce calls the stimulus of the unlike probably operates in modifying handwriting as well as other expressive functions.

Tests on modified writing as used by Rowe (9) in studying the control of voluntary movement and by Ordahl (7) in a study on the factor of consciousness in the learning process have emphasized certain points concerning the psychology of handwriting. Rowe found that writing with the left hand, with writing reflected in a mirror, showed such a strong tendency to follow the visual cue that it forces the conclusion that, normally, vision assumes the chief rôle in writing reactions, especially while such reactions are being learned. Again, in upside-down writing the difficulty was in visualization of the letters so that, in general, the conclusion is drawn that a change in the visual element causes greater interference with writing activity than when the muscular element is changed (not eliminated). Attention in highly volitional tests is upon the extra-bodily aspect, but in comparatively automatic writing the visual control tends to lapse, and resident or remote sensations giving awareness of the position of the moving parts alone remain in consciousness. Thus in upside-down writing there is visual attention, but in copying nonsense syllables while repeating the German or Greek alphabet there is a minimum of attention to result. Writing, however, rarely becomes completely automatized, so that it suffers without slight visual supervision. Inner speech, Rowe thinks, is distinctly a cue when a familiar copy is written under easy conditions; when the conditions are difficult it is a mere accompaniment. Ordahl found that in learning to write with the left hand and in learning to write mirror-script with both hands consciousness functioned chiefly in perfecting adjustments which were at first 'unconscious,' then highly conscious and, finally, automatic.

The fact that estimates of handwriting quality are vague in the

extreme and shifts in standard frequent has led Thorndike (13) to attempt the construction of a graphometer or scale which may be used in measuring the quality of a given handwriting. To construct such a scale involved the collection of a thousand samples of writing and the passing of judgments upon them by thirty or forty competent judges of handwriting, from the average of whose gradings the relative merit of different samples may be found and a scale constructed. Such a scale Thorndike constructs for measuring the quality of handwriting by children in grades 5 to 8 and a similar scale for quality-determination of the writing of adult women. Actual samples, representing points on both scales, are published so that, if desired, the quality of any given specimen may be readily measured by comparing it with the samples given. The use of such a scale would undoubtedly be of value where estimates of handwriting quality are to be reached as in teachers' or civil service examinations. Such a scale would also be of value in attempting to determine the value of different methods of teaching and the amount of time to be spent on writing. From a comparison of the quality of handwriting in children and adult women teachers, Thorndike concludes that the school systems develop a higher quality of writing in some children than is practically necessary and that when children reach a certain point on the scale of handwriting, they may profitably be released from further instruction.

Thorndike in the same article reports an investigation on the speed and quality of handwriting as dependent upon difference in means and methods of teaching handwriting, and reaches the general conclusion that, in the school systems investigated, differences in methods of instruction and the time devoted to the subject "are of considerable influence upon efficiency." Individual differences in both speed and quality of writing were found to be very great. Rapidity is in and of itself held to be a sign of ability, but Thorndike found that the correlation between quality of handwriting and grades in scholarship is zero.

The pedagogical interest in handwriting appears also in Lindner (6), who attempts to determine from a psychological study of the various forms of the verbal idea, both sensory and motor, the best method to pursue in teaching a child to write. He concludes that the writing activity should be begun only after the reading activity is well initiated, basing this opinion on his conclusion from pathological evidence that the motor centers are subordinate to the sensory. He urges beginning writing expression with the word rather than

with the letter and emphasizes the value of allowing individual preferences to operate in the manner in which certain details are learned. That the learning process in writing affords interesting material for determining the way in which attention works in the analysis of the word is evident from the examples Lindner cites.

Hornig (2) reports observations on the spatial dislocations found in the writing and drawing of children in a chosen primary grade. The particular children whom he observed,—all afflicted with speech defects but otherwise normal,—gave very frequent examples of such dislocations in the form of inverted or reversed ciphers, letters, or drawings. Such inversions did not seem to be a sign of inferior intelligence. There are no data by reference to which we may determine whether such spatial dislocations are found more frequently in such children as Hornig observed than in the normal child. It is very difficult to get the child to see the difference between his production and the copy, but such errors may be avoided by careful preparatory exercises in the movements used in writing and drawing.

Kretzschmar (5) continues his report on work in progress at the University of Leipzig relative to a comparative study of children's drawings. The Lamprecht collection of 100,000 drawings from European children and some 12,000 from children of other regions of the world furnish an immense amount of material to be worked over in determination of environmental, racial and developmental factors in the life of the child. In spite of different conditions of growth, will a comparative study reveal similar stages of mental development? The brief report is concerned mainly with descriptions of the drawings of children of the red, yellow and black races. The collection contains 2,500 drawings from Japanese children,—drawings of individual objects, animals, men and landscapes, the latter drawings being particularly valuable in a comparative study of the representation of perspective. The collection of drawings from children of the tribes of North American Indians is a small one and stands greatly in need of supplementation. In the samples obtained there is shown an extraordinary love of bright colors and of ornamental devices; no drawings of landscapes or of human beings appear in this collection. The collection of drawings from the east and the west African negro is particularly valuable, being supplemented in the former case by modelled figures. This latter being the native form of expression, it is possible to draw a valuable comparison between it and the drawing activity which is acquired by instruction in the schools.

Ruttmann (10) contributes a critical résumé, with a selected bibliography, of the experimental researches that relate to the psychology of drawing and gives a comprehensive exposition of the methods that have been used in such work. In particular, he discusses the statistical, monographic, ethnological, biographical, and experimental-æsthetic methods. Although the mass-experiment is frequently fruitless because of the incompetence of observers and the failure to define rigidly the conditions and the aim of the investigation in progress, yet the statistical method, if applied critically, is basal to all comprehensive work. Monographic work, although deficient from the side of comprehensiveness, is definite in aim. The comparison of the development of the drawing activities of the child with the development of the same activity in the race and in the artist, affords opportunity for valuable deductions. Biographic material can be used with profit if care be taken to guard against reading such material in the light of later experience. It appears to be difficult to secure records of spontaneous drawings, but such records are most desirable. The application of the experimental methods of psychology to æsthetics promises a rich development which will serve in many ways the psychology of drawing. Ruttmann summarizes the results of experiments relative to the stages in the development of drawing activity in the child preceding and subsequent to the beginning of school life. The child's early preoccupation with the human figure is reaffirmed and the characteristic feature of such drawing detailed. Stages in the development of the perception of perspective and color are also noted in connection with the development of space perception in the child and the relation of this development to the spatial dislocations found in the drawings of children. The effect upon the free drawing activity of the child of the customary instruction in the schools is considered; it is frequently true that such instruction has a negative effect. The difference in the drawing aptitude of boys and girls as shown by investigation is considered, with an inclination to interpret this difference as a fundamental sex-difference. A valuable section of the monograph takes up the development of drawing activity in the gifted individual, although graphic skill is not always correlated with artistic gifts. Kick's interesting comparison of drawings motivated by the desire to copy nature and those originating from desire to express the image is given. Psychopathology is also found to furnish material of value in interpretation of the drawing activity through exhibiting the results arising from the failure of certain functions of attention. Levinstein's

comparison of ontogenetic and phylogenetic development, based on the Leipzig collection of drawings, is summarized. In conclusion, the author reviews Albien's investigation on individual types as manifested in the drawing activity and commends the attempt to reach a psychological analysis of the drawing act by experimental means.

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VOCAL FUNCTIONS

BY PROFESSOR W. VAN DYKE BINGHAM

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What is the internal mechanism of vocal pitch regulation? Berlage (1) has sought to separate out from among the complex factors of control any influences due to the articulatory movements of the larynx itself in regulating the pitch and of the mouth in forming the vowels. With a form of Krueger's 'Kehltonschreiber' records were taken of the pitch of the voice in repeating a tone, sung previously

either by the reagent himself or by another. It was found that accuracy in imitating the pitch of another's voice decreases regularly with increasing length of interval between standard tone and repetition, up to 30 seconds, the maximum interval used. Accuracy is greatest when the pause is from one to two seconds: here the two best reagents had average variable errors of only 0.5 and 0.6 vib. Self-imitation is more accurate than the repetition of another's tone. Throughout a wide range of standard pitches, the magnitude of the slight fluctuations of pitch remains approximately constant when expressed in percents of the vibration frequency. Within the course of a single tone held for three seconds the deviations from the average pitch of that tone show no regularity, except that the pitch during the first tenth of a second is too low, as Cameron found, and further, certain reagents regularly make a sudden correction of pitch somewhere between the fourth and the twelfth tenth-of-a-second.

The most interesting point brought forward is that accuracy is impaired by the assimilative effect on the tone-idea of the formants contained in the vowels. Thus in singing *i* after singing *u* (sounds as in *sie* and *du*), there is a tendency to make the second tone more flat; while in singing *u* after *i*, a tendency to sharp the tone appears. The mental structure of the sung tone is, then, a complication of two elements relatively independent but assimilatively influencing each other: sensations corresponding to the pitch of the sound, and sensations corresponding to the 'vowelization.'

Berlage holds that peripherally aroused auditory sensations do not directly participate in the regulation of the pitch of the voice tone, the direct control being accomplished by means of auditory images and kinesthetic sensations. Regarding the influence of articulatory sensations on tone experiences in general, he takes a middle ground, finding it impossible to say with Stumpf that they are of no significance.

While the methods and apparatus of Seashore and Jenner (4) are beautifully adapted to the solution of problems regarding the structure of a tone-idea and the analysis of the mechanism of voice control, their purpose in the present article is limited to the illustration of a method of experiment in the field of the acquisition of vocal skill. The reagents were tested in accuracy in (*a*) striking a required pitch, and (*b*) making a least-producible voluntary variation from a standard pitch. They were than given ten periods of practice, with the visual aid furnished by the tonoscope, and finally re-tested, in order to determine the amount of improvement which carried over

from the practice singing 'with aid' to singing without the aid of the visual control. That this transfer takes place to a considerable extent is not clearly evident from the tables, except as regards ability in controlling the minimal variation of a sung tone. The fact that this paper reports preliminary experiments doubtless accounts for the fact that no use was made of the device of giving to a group of control reagents a similar amount of practice without the aid of the tonoscope. The reports of more extended experiments by these writers will be awaited with keen anticipation. If their apparatus were in use in several of our laboratores, knowledge regarding 'vocal functions' would advance with rapid strides.

Can it be that the muscles of the back are important determiners of vocal 'clang-character,' as some eminent voice teachers maintain? According to Rutz (3) vocal compositions are of three main types, demanding for effective rendition a tone that is 'dark and soft,' or 'clear and metallic,' or 'clear and very soft' (terms descriptive of quality, not intensity of sound). To secure the desired clang-character, the singer or speaker must maintain a specified characteristic position of the muscles of abdomen, chest and back.

Krueger (3) in discussing this much debated theory brings forward some neglected physiological considerations. The innervation of the larynx, through the vagus, and of the organs of articulation, through the fifth and seventh cranial nerves, is functionally bound up with the innervation of other organs, notably the ear, the diaphragm, the face muscles and the muscles regulating bodily posture. Numerous interconnections are traced with both sensory and motor portions of the eighth, ninth, eleventh, and twelfth nerves and with the sympathetic system. Hence the likelihood that stimulation of a given musculature may produce 'sympathetic' movements (Mitbewegungen) of apparently unrelated musculatures. Moreover, many of these movements through their direct and indirect sensory effects serve to modify on-going emotional processes. The pedagogical worth of the Rutzian theory lies in the fact that it 'indicates a point of attack for the will.'

Weld (5) has brought together some of the most elementary facts regarding the mechanism of the voice and its hygiene. Zwaardemaker's (6) masterly survey of the applications of experimental phonetics to medical problems contains a valuable bibliography.

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SPECIAL REVIEWS

THREE TEXT-BOOKS OF PSYCHOLOGY

Essential of Psychology. W. B. Pillsbury. New York: The Macmillan Co., 1911. Pp. xi + 362.

Introduction to Psychology. R. M. Yerkes. New York: Henry Holt & Co., 1911. Pp. xii + 427.

An Introduction to Experimental Psychology. C. S. Myers. Cambridge: The University Press, 1911. Pp. vi + 156.

The appearance of two new text-books of elementary psychology written by experienced teachers will be welcomed by all who teach the subject. These books show marked contrasts in viewpoint and arrangement of subject matter and to a less degree in the material presented.

Pillsbury's book in most respects conforms to the same general plan and presents the same material as most of the text-books of elementary psychology that have appeared in recent years. The introductory discussion is, however, built upon a somewhat novel definition of psychology as the 'science of human behavior.' This definition, at once so broad and so narrow, does not greatly modify the later treatment, the terms 'behavior' and 'consciousness' being used as practically interchangeable. Following the introductory chapter there appears a chapter on the nervous system, which is described as composed of three levels of sensori-motor arcs. This conception of the nervous system has been made familiar by McDougall, but Pillsbury's description differs from that of McDougall in making both of the two lower levels subcortical. Considerable emphasis is placed upon the synapse, and throughout the book the author makes free use of his view of the probable function of the synapse in explaining such processes as habit and association. Another respect in which the book differs from most texts is in placing a chapter on retention and association between the one on sensation and the one on perception, this being done because the former processes are, in the author's opinion, necessary in explaining the latter. Besides the ordinary topics the book includes chapters on work and fatigue and on the interrelation of mental functions, the latter treating the subject of formal training. Some of the newer experimental material, espe-

cially in the field of memory, is found in the book. Each chapter is followed by exercises which seem to be carefully selected and should add greatly to the value of the book.

One feels from the reading of Yerkes' work that the author has revolted strongly from the usual methods of the text-books as a result of his teaching experience. The book deviates greatly from the beaten path. The author's first concern is that the student should adopt the strictly psychological point of view and that his attitude towards psychological facts should be wholly scientific. Much space throughout the book is devoted to defending the scientific nature of the subject and comparing it with other sciences. A larger proportion of the book than is usual is devoted to the introductory discussion which constitutes Part I. The introspective point of view is insisted on at every turn and is emphasized especially in the quotations and class exercises at the beginning and end of each chapter. No description of the nervous system is given, since it is the author's purpose to adhere strictly to pure psychology, and since the presence of physiological material in a psychological text-book obscures the true nature of the work. The plan of the book differs largely from that of the ordinary texts. Part II., constituting the main body of the work, gives an analytic description of consciousness. Part III. gives a genetic description. Part IV., entitled Psychology as Generalization, in which the author enumerates some of the important psychological laws, is especially suggestive. Part V. deals with explanation and correlation. Here the author argues strongly for the explanation of psychological phenomena through psychical causation. Part VI. contains chapters on the prediction and control of events, on education and the mental life, and on eugenics and the mental life.

Myers' work constitutes the volume on experimental psychology of the Cambridge manuals of science and literature. It is in no sense an abbreviation or condensation of his larger work on the same subject; nor is it a general survey of the field, but deals in a concise and clear way with a number of subjects likely to be of interest to the layman. The topics include touch, temperature, pain, color vision, the Müller-Lyer illusion, experimental æsthetics, memory, mental tests and their uses. The chapter on color vision is especially interesting, being accompanied by two colored charts, borrowed from a forthcoming work of Hering.

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FITE'S INDIVIDUALISM

Individualism: Four Lectures on the Significance of Consciousness for Social Relations. WARNER FITE. New York and London: Longmans, Green & Co., 1911. PP. xix + 301.

Professor Fite's *Individualism* is a notable discussion of the conceptions of the individual and society in their reciprocal relations. The remarkable lucidity of the author's style and the simplicity and deftness of presentation have given the book an immediate and sympathetic audience. With the social theory the BULLETIN is not immediately concerned, but in the argument that supports the theory the psychologist is interested, for the theory of consciousness presented, for the criticism upon functional psychology, and for the detailed criticism which the author makes upon the positions of Tufts and Dewey in their *Ethics* and upon James' statement of the instincts, and the object of conscious desire.

The principal thesis of the book is that conscious conduct is always that of an individual who acts from motives of self-regard.

Consciousness, however, implies that all the aims of the individual should receive consideration, and that in proportion as the act is fully conscious all will be satisfied in an act which is deliberately chosen. Consciousness furthermore implies that the environment within which conduct takes place is elastic and in proportion to the completeness of the consciousness of the actor is fully amenable to the purposes that are seeking expression. This involves not only the comprehension of the environment—as this would be given in a perfect science—but also the freedom of the conscious individual, a freedom which lifts him above material law and a mechanical order of nature.

It implies further such a complete comprehension of the social environment that the individual who acts with reference to others will fully recognize their individual natures, their aims and ends. This recognition involves the acceptance of the nature of the 'others' who make up this social environment, as of like and equal worth with himself—as ends in themselves—whose peculiar character he can use only in so far as he intelligently estimates and values their natures.

On the basis of such an individualism the author maintains a theory of natural rights, of social contract, and identifies justice and brotherly love. The author's definition of consciousness is diversity in unity. This is placed over against a definition of the mechanical object. The mechanical object has, as such, no internality. In itself

it has no relations—is merely acted upon from the outside. This object has therefore neither diversity nor unity. So far as it falls into a system this relationship with other things exists only for an outside intelligence. The operation of natural law upon such elements—such atoms—is necessary and mechanical. Mechanical objects exist only in space and time, occupy isolated points and moments, and in themselves imply neither other places nor other moments.

An implication of the 'there' in the 'here,' of the 'then' in the 'now,' can exist only in an idea in a mind. Diversity in unity, or unity in diversity, then, can exist only in consciousness, and wherever these are found, they must be found in consciousness. Professor Fite insists that such a formal definition of consciousness is legitimate and criticizes those who, like Wundt, insist that consciousness may not be defined. While this formal definition undertakes to distinguish consciousness from the unconscious, it does not state the content of consciousness, which involves not simply the presence of all elements in relation within the idea of the relation, but also the aims and values of these elements. In other words, consciousness is not only unity in diversity, but is the unity of impression and response, of conation and satisfaction, of feeling and emotion.

But the author does not admit that this unity arises, in any way, from the activity implied in animal organisms. As consciousness it can have no origin. In a word, Professor Fite denies the functional doctrine of consciousness. For him it can be neither stated as the process of selection of appropriate stimulations and responses, nor as the adjustment of conflicting tendencies. Consciousness with reference to the organism simply *is*. When it is, it at once arises above the mechanical process of stimulus and response, and becomes free.

Now this doctrine, which is metaphysical and not psychological, is used by the author to justify his position that the process of consciousness, so far as it is present, includes always both elements in the subject-object relation. There is a growth in the degree of consciousness, not a growth in consciousness itself. So far as we are conscious, we are conscious of objects and of ourselves as subjects. Consciousness may increase in so far as the field of systematic knowledge is concerned, and our subject-consciousness will increase in proportion; but the relation of subject and object must be given in every conscious state, for this is what consciousness is. This is not the only use made of the doctrine of consciousness by the author. Its first and most considerable purpose is to overthrow a mechanical

theory of the individual in society, but when Professor Fite reaches the discussion of social consciousness and the object of conscious desire, he has as his presupposition a consciousness in which no object can rest without an equally clear and distinct subject, a consciousness that in its nature is an idea of a relation within which exist both elements in the relation, and this presence of objects in relation is not the result of the development of consciousness, but is the nature of consciousness.

So the author states that the young child has, to be sure, inadequate self-consciousness, but that it has correspondingly inadequate object-consciousness. The child cannot, therefore, come to consciousness of others first, nor consciously imitate others before he has a consciousness of self. His self-consciousness must be strictly contemporaneous with the consciousness of others. In like manner so far as he consciously desires an object he must do this with conscious reference to the self that must be equally present with the object of desire; for it is the nature of consciousness to be such a unity of diverse elements. At least it is to be presumed that this definition of consciousness determines the author's theory of the self, for he simply asserts that such equal recognition of the self with the other is implied in consciousness.

From the standpoint of the empirical psychology that the author refers to somewhat comtemptuously, it is quite conceivable that conscious attention should be occupied with the field of stimulation, should make discriminations within this field, should organize contents in objects without having for the time being a corresponding consciousness of the subject. It is the teaching of our empirical psychology that attention and apprehension are occupied at first with the stimulus and only gradually get conscious control over those elements which constitute the content of self-consciousness. From this standpoint it is perfectly conceivable that 'others' should arise in the consciousness of the child before an equally clear presentation of the self appears. In the same manner it has seemed quite reasonable that conscious beings should be consciously desirous of objects without referring this desire to themselves in the same act of consciousness.

This position of James, which Dewey adopts, the author combats simply by insisting that consciousness must necessarily present the subject of desire as definitely as the object of desire. The author's position is, not that such a relation is the implication of desire, but that it is inconceivable that a conscious being should desire an object without referring the object to himself as the object of his desire.

He admits that a hungry man may seize bread and devour it without consciousness of self. But such an act is in so far not a conscious act.

The position becomes still more important in the criticisms of the *Ethics* of Dewey and Tufts. He accuses them of subordinating the individual to society, because these authors have maintained that moral development is a process by which a larger self arises through the consciousness of the social ends.

In their historical treatment the appearance of self-consciousness is shown to be the point at which the old group morality breaks down. This new form of self-consciousness becomes the starting point of the development of conscience. As the values of group life appear in opposition to this self-consciousness, it becomes morally necessary that a larger self should arise to meet the enlarged demands of society on the individual. Now this development of the larger self is a process within which, according to Dewey and Tufts, the conception of a social good is formed earlier than the conception of the self that answers to this larger duty. It is through the formation of the new moral and social object that the larger self comes into existence.

Professor Fite interprets this doctrine to mean that the self is to be sacrificed to society, that the good of the self is to be ignored over against the common good.

It is true, according to the doctrine which these authors embrace, that the former self is to be in a sense sacrificed. And quite in accordance with the theory of object- and subject-consciousness stated above, the new object must be presented before an adequate conception of the self can be presented. But this sacrifice of the former self does not in any sense imply that the self as such is sacrificed, indeed it implies that a larger and more adequate self supersedes the earlier self,—a self which commands the same respect and right that belongs to all others in the social group. It is only a doctrine which assumes that consciousness of the object must be at once coterminous with consciousness of the subject that could maintain that this theory subordinates the self and its good to that of society and its good. It is true that reflective moral conduct is constantly engaged in such adjustment of the self to an enlarged sense of moral obligation, and that, therefore, in reflective moral consciousness the attention is centered upon the construction of a new objective world and that the consciousness of self follows upon this. But this reflective moral consciousness is not the whole of consciousness, nor are we continually engaged in reconstructing our moral universe.

In this presentation of Professor Fite's Doctrine of Consciousness

and its corollaries I have sought to bring out only this one point of central psychological interest, that subject and object must be strictly coterminous in consciousness, and that, therefore, the individual cannot come to self-consciousness through the consciousness of others, nor can desire center upon the object without reference to the self, nor can moral consciousness present the common good as its object before the larger self arises in consciousness which is implied in this common good.

For the author of *Individualism* to assume that the object can appear in consciousness earlier than the subject, and determine the conditions under which the subject will itself arise, is to assume that the subject disappears entirely, is sacrificed to the object, and its values ignored. The authors of the *Ethics* which he criticises, on the contrary find in this process, not only conscious moral growth, but the growth of the larger self as well, a self which seems to them as highly important and valuable as does the self to Professor Fite.

Professor Fite also recognizes the necessity of a growth in the self. He suggests, however, no process of growth. Indeed from the point of view of his thesis of consciousness it is hard to see how the self could grow consciously. It must always be there, and moral consciousness can be but a realization of relations of values already in existence, not the formation and acceptance of new relations and values, which lead to the conscious formation of a new self arising to meet new obligations. It further strikes the reader of *Individualism* that its author recognizes no motive that can lead the narrower self to abandon its attitude and values and accept those which belong to the larger self. If a larger object is there, either the narrower self will necessarily fail to appreciate it, or the larger self by definition will be already present. There is no room for the moral effort which Professor Fite himself implies. His doctrine is in spite of himself hopelessly optimistic.

An important corollary of this position is that the doctrine of *Individualism* suggests no method of conscious moral progress. It is an analysis of an achieved social organization, not a method of social advance and reconstruction. If Professor Fite had realized this more fully he might have been less severe in his strictures upon the social theorists, whom he calls 'Anti-individualists,' and who with varying degrees of success are attempting to determine the social conditions under which a larger moral self-consciousness can arise.

It may be pertinent to add that society is discovering that by changing social conditions it may to a considerable extent determine

the character of the selves that go to make up society, and that the moral and social theory which Professor Fite assails is attempting to make a method of this process for individual consciousness.

In conclusion the reviewer must again insist that he has considered only a single though central thesis in *Individualism*, and that he has been quite unable to do justice to the wealth of acute analysis and admirable presentation of which the book is full.

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ETHICS

Grundzüge der Ethik. E. DÜRR. Heidelberg, 1909. Pp. xxvi + 383.

This book is the first volume to appear of a series of psychological monographs (*Die Psychologie in Einzeldarstellungen*) projected by the late Professor Ebbinghaus, before whose death some fourteen volumes were arranged for, with Dr. Meumann as colleague, under whose editorship the list has been carried to twenty titles. The series is significant if for no other reason than that it marks the passing of the day when psychology can make progress by the giant labors of the encyclopædic genius, and, like the other sciences, must depend upon the trained abilities of the specialists. In its title, however, it differs from the plan announced some years ago by Professor Baldwin in which an attempt was to be made to develop systematically the main lines of psychological doctrine. The German series does not aim to develop a systematic doctrine of psychology, but to enter the several fields of humanistic knowledge and consider these from a psychological point of view.

This book of Dr. Dürr's has grown out of certain investigations that the author has made of feeling and will, and certain other philosophical inquiries into the reality and meaning of the moral ideal in its individual and social aspects. In addition to these qualifications for his book, Dr. Dürr has brought to bear upon the problem a knowledge of the main historical systems, and it is in connection with a critical review of these that he advances to a statement of his own systematic view. This procedure may be recommended to our American philosophical fraternity among whom perhaps there is a tendency to undervalue the work of the classical writers in the several departments of knowledge. The progress we make in coming to a more thorough understanding of ethical and religious values will in some large measure depend upon our ability to combine the three qualifications that Dr. Dürr brings to this work

—psychological training, historical knowledge and speculative interest.

In the Introduction, after a few pages given to the distinction of ethics, morals and moral instruction, the author raises the question of the objects of ethics. These he finds in the feeling and volitional life of the moral subject. Moral feeling, he tells us, is a capacity for moral judgment: this it is before anything else. But it is not only this: it is also the foundation of moral will and action. This distinction between moral judgment and will, we are warned, does not mean that there are here two faculties. "Moral feeling as the capacity for moral judgment and as the capacity for moral will and action," the author writes, "does not indicate a sum of two faculties, but considers one faculty under the point of view of different activities." In this way we are prepared for the statement that moral judgments are judgments in which the predicates are worth-designations, by which is meant that they point to a relation of objects to our feeling life. "What they indicate is a relation, and ordinarily a causal relation of objects of worth to our feeling, or more strictly a property of objects through which it is enabled to excite our feeling life. What it expresses is thus this excitement of our feeling life." It is, of course, possible to affirm such predicates as good, bad, etc., without thinking very much about it. But for the ethicist these value judgments are the objects of supreme interest. "What interests the ethicist in the moral judgment above everything else is moral valuation." In addition to the value judgment, there is the question of the motive of moral valuation. If, therefore, morality is described as valuation and its motive, we must bear in mind that valuation itself can become the object of moral judgments, and any object of moral judgment is a value. These considerations mark out in a preliminary way the main lines of inquiry, namely, (1) morality as valuation and the disposition thereto, conscience; (2) motivation of morality, moral action and character; (3) morality as value and criticisms of the ethical systems.

Apart from the Introduction, there are five chapters in which these main problems are developed. In Chapter 1 are discussed the nature, object, conditions, forms and effects of valuation, and also a determination of the nature and conditions of conscience. Moral will and action, involving the problem of freedom and responsibility which are treated from a deterministic point of view, are considered in Chapter 2, and the whole discussion leads up to a statement of the meaning of character. Chapters 3 and 4 bring the

points of view of the two previous chapters to bear upon the question of valuation in the individual and the human species. In Chapter 5, on Morality as Value, through a statement and criticism of the systems of morality the author prepares the way for the presentation of his own constructive view which he denominates 'egocentric rigorism.' In this system, conscience is the supreme value, not however as an end in itself, but as an instrument of realization. It has both an individual and a social function. In this way he is able to deny that there exist freak consciences (Sondergewissen) and hence a morality outside of the moral community. He recognizes, however, that the human life of the individual is much richer than any system of morality however complete, so that there is room in the moral life no less for men of action like Bismarck, than for men of principle like Kant.

The book may be recommended as a particularly pleasing contribution to the discussion of ethical problems. It is written in a style that places no unnecessary obstacles in the way of the foreigner, and the notes to the chapters, which have been grouped together at the end of the book, are particularly rich in bibliographical material.

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DISCUSSION

THE UNCONSCIOUS BIAS OF LABORATORIES

In my recently published review and critique of the work centering about the problem of the content of thought,[1] I have attempted to show that the failure of certain investigators to discover imageless factors might be attributed to "a predisposition on the part of the 'trained' observer which is decidedly prejudicial to the discovery of *meanings* in experience." This point I have illustrated, in a striking manner, as I thought, by reference to the analyses of conscious attitudes made by Miss Clarke. Miss Clarke's subjects, I have said, "lay all stress on the sensory factors in their experiences, and whenever meaning factors appear, they are either passed over by the author, or referred to as *Kundgabe*. It is interesting to note, however, that in the analyses given of some sixty attitudes such as approval, caution, difficulty, surprise, etc., there appear several which, as named, clearly involve an element of *knowing*. For instance, the

[1] Cf. this journal, 8, 183–197.

'consciousness that the letter was too small,' 'fear that I had reacted too quickly,' 'I ought to know that,' etc. Yet each of these is unblushingly analyzed into 'muscular strain and organic sensation,' 'slight sinking of the stomach and diaphragm,' and the like."[1]

My attention has been called to the use of 'unblushingly' as seeming to impute dishonesty on the part of Miss Clarke in handling her data. This has surprised me greatly, for I had no thought that the 'predisposition' of which I accused Miss Clarke and others should be interpreted otherwise than as an unconscious determining tendency which led always to the discovery of sensations and never to the discovery of thoughts.

It is my belief that when one is predisposed to the discovery of *meanings*, sensations become secondary phenomena, and when one is predisposed to the analysis of sensations, meanings are not focal. It is impossible, I think, that both should be focal at the same time, hence the discrepancy in the results of two sets of investigators each working on the same problem but with fundamentally different biases. The average 'trained' observer of our laboratories is one whose training has consisted in manifold analyses of sensation and images. These are the things for which he will naturally look, and without a counter suggestion, these are the only factors which his analysis may be expected to reveal.

I wish publicly to disclaim any intention of accusing the Cornell investigators or those of any other laboratory of wilful distortion of the data which they have obtained. Furthermore, I have never harbored the thought that undue influence was exerted on these investigators by the attitudes and beliefs of the directors of these laboratories. But surely, in problems as delicate as these, it is not overt acts on the part of director, investigator or observer with which we have to deal, but rather with attitudes which unconsciously pervade the laboratories and bias accordingly the work which is done there. I doubt if any laboratory can be entirely free of such a prejudice. I have taken pains in my review to call attention to at least two instances in the work of Binet and Moore, respectively,[2] in which a predisposition favorable to the imageless content has, I believe, led the authors to conclusions which the data do not unequivocally support.

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[1] *L. c.*, p. 194.

[2] *L. c.*, pp. 188, 191.

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NOTES AND NEWS

THE First International Congress of Pedology, held at Brussels August 12 to 18, was attended by about six hundred members. Dr. O. Decroly of Brussels presided, with Professor M. C. Schuyten of Antwerp as Honorary President. Professors De Sanctis of Rome and Professor H. C. Warren of Princeton were Honorary Presidents of the section on Child Psychology. The papers in this section were of particular interest and it is to be regretted that few Americans were present.

PROFESSOR HUGO MÜNSTERBERG has returned from Berlin, where he has been spending a year as exchange professor.

PROFESSOR SANTE DE SANCTIS of Rome has obtained for his laboratory the entire equipment of the late Professor Z. Traves of Milan.

HENRY FOSTER ADAMS, Ph.D. (Chicago), has been appointed instructor in psychology at Michigan University.

THE following items are taken from the press:

DR. W. G. SMITH, formerly professor of psychology at Smith College, has been reappointed Combe lecturer in psychology at the University of Edinburgh.

FUNDS are being collected for the purpose of erecting a monument to honor the memory of the late Professor Cesare Lombroso, at his native place, Verona, Italy.

EDITORIAL NOTE

At a conference this summer between representatives of the *Psychological Review Publications* and the *Zeitschrift für Psychologie*, a uniform classification was agreed upon for the Psychological Index and the Bibliographie published by the latter journal. The contents of the two bibliographies will henceforth be practically identical. The new scheme involves considerable change in the classification of each, but it is believed that the initial difficulties attending the use of a new division of topics will be more than offset by the improvements introduced and the need of referring to one bibliography only.

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THE PSYCHOLOGICAL BULLETIN

GENERAL REVIEWS AND SUMMARIES

CHILDHOOD

BY DR. THEODATE L. SMITH
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Child study has become within recent years so large a field, with so many diverse ramifications, that in any attempt to represent it within a limited space, of necessity only a small part of the actual literature can be presented. The most that is possible is to give sufficient typical examples in the more important lines of development, that an idea of the scope of the subject may be furnished to the reader.

General Treatises.—The new edition of Professor Tracy's well known *Psychology of Childhood* (Boston: Heath, 1910, pp. 229) is the seventh English edition, which was intended to appear simultaneously with the German translation brought out by Professor Stimpfl in 1908 but delayed by the illness of the English author. The book is too well known to American readers to need more than a note on the recent additions. The earlier text has been thoroughly revised, and Professor Stimpfl has added paragraphs throughout the book which increase its interest, as well as a seventh chapter which deals chiefly with the abnormal mental conditions of child life, the study of which has assumed such important proportions during the last decade. This adds greatly to the value of the book since it discusses problems which the teacher is constantly meeting. As a text-book, it remains what it has always been, a standard.

Perhaps one of the most stimulating books on the child which has appeared in recent years is Ellen Key's *Century of the Child* (New York: Putnam, 1909, pp. 339) which, having passed through numerous editions in Swedish, German, and French, was made accessible to the English reading public in 1909. Though some of the author's

ideas are revolutionary, it must be remembered that the position of woman in Sweden is so different from that in America, that the author's attitude in regard to some questions can not be fairly judged without a full knowledge of those conditions. Certainly no one can mistake the ethical purpose of the writer nor fail to appreciate her originality and keen insight into many of the problems connected with child life. The topics discussed are the right of the child to choose his parents, the unborn race and woman's work, education, homelessness, soul murder in the schools, the school of the future, religious instruction, child labor and the crimes of children. The chapter on education has been reprinted in separate form.

The title of Miss Higgs' book, *The Evolution of the Child Mind* (London: Froebel Soc., 1910, pp. 67) might possibly be misleading to students of psychology who were looking for a study of intellectual development, since it is a study of moral development from the kindergarten point of view. The fact that it is published by the Froebel Society will serve to introduce it to kindergartners though it will of course be equally helpful to parents.

The translation into German of Paola Lombroso's *La Vita dei Bambini* (Leipzig: Nemnich, 1910, pp. 112) has greatly increased the accessibility of a book well known to specialists in child study since 1904. The author, thoroughly conversant with the literature of the subject dealing with the facts of child life, failed to find in them a law or principle which should coördinate all the varied manifestations of child activity and set herself the task of finding out this principle. This fundamental law of child psychology which seems to her to coördinate and explain all the varied mental and physical activities which pertain to the child, and which is the essential idea set forth throughout the book, is "the strong sense of conservation and self protection, the jealous care with which the child seeks to preserve himself from everything which can hinder or impede, even in the slightest degree, his development." It is of course recognized that this instinct of individual protection and conservation is also characteristic of adults, but it does not exist in so exclusive and restricted a form as in the child. The child wills imperiously to live and this impulse is deeply rooted in his individual life. Between this purely organic instinct in the child and the instinct of social conservation in the adult lies a wide chasm. The adult voluntarily subjects himself to fatigue, endures difficulty and perils, sacrifices rest and energy to attain his end; children, on the contrary, pursue an entirely different line of conduct, shun effort and fatigue and

eagerly seek enjoyment under whatever form it is presented. Not to be forced, not to waste energy, to avoid continuous work, to shun fatigue and difficulty, to gather for himself the greatest sum of pleasure, to manufacture enjoyment out of the most monotonous and indifferent things, the author believes is the great law of child psychology through which the child establishes himself strongly and tyrannically in life. It should be noted that the Italian word translated *Kind* is generally used to cover the period from birth through the sixth year, so that it would be an injustice to the author to apply the above picture to older children. The first two chapters are devoted to an exposition of the principle above given; succeeding chapters discuss the evolution of thinking, children's drawings, and the psychology of the children of poverty, in which discussion the influence of environment is strongly brought out.

In Vol. 77 of the *Popular Science Monthly*, Professor Kirkpatrick has given a brief but excellent sketch of what is included in child study. The presentation is intended to be popular and of necessity is restricted to a mere outline, but it indicates clearly and effectively the value of child study and calls attention to some of the newer phases of its development along the lines of child welfare.

Bouquet's *L'Evolution psychique de l'enfant* (Paris: Bloud, 1910, pp. 101) is a convenient little outline of child psychology, sketching briefly the psychic condition at birth, development of the senses, walking and language, and then passing to the more purely psychological questions of habit, memory, pleasure and pain, fear, imitation, imagination, fetichism, and finally moral evolution. While the book adds nothing new to our knowledge of the psychological development of the child, it is a brief presentation of the subject which will appeal to many readers.

Here too should be mentioned Fritzsch's contribution to the early history of child study (*Zsch. f. päd. Psychol.*, 1910, 11, 149–160), in which he shows that Tiedemann did not stand alone but was one member in a series of pioneers in child study in the second half of the eighteenth century, and is closely bound up with the great pedagogic revival with which the names of Rousseau and Basedow are associated. In the emphasis laid upon the pedagogical side of the work of the great educational reformers, the fact has been overlooked that they were really forerunners in the field of child study, especially as represented in the recent development of experimental pedagogy.

Two recent books which, though they are perhaps more strictly

classified as practical ethics than child study, are so thoroughly based upon it that no general report on the subject can fail to include them are Edith Read Mumford's *The Dawn of Character* (New York: Longmans, Green, 1910, pp. 225) and Edward O. Sisson's *The Essentials of Character* (New York: Macmillan, 1910, pp. 224). Both books show a wide and sympathetic understanding of children and are based on sound psychological principles, theoretical discussion of abstract ethical ideas being conspicuously absent from both. It is a rare occurrence that two books of such unquestionable excellence on the same subject appear within so brief a period.

The work of the Polish Child Study Association, which is reported by Madame Grudzinska (*Ped. Sem.*, 1910, 17, 119–120), is of especial interest, coming as it does from a country where political conditions have been such as to repress scientific movements of all kinds. In spite of this, however, Madame Grudzinska writes that "child study as a special branch of psychology has in recent years awakened much interest in Poland" and the report of the work of the association certainly confirms the statement.

Individual Studies.—An excellent record of the infant development of her two sons has been published by Mrs. Tilly (*Assoc. of Col. Alum. Mag.*, Series III., No. 22, 1910) and adds one more to the small but valuable collection of individual studies in this field.

Paolo Vecchia has also contributed a similar record of two children, a boy and a girl (*Riv. Ped.*, 1909, 3, 21–41, 134–152).

Another contribution is Irving King's careful and systematic measurement of the growth of two children (*J. of Educ. Psychol.*, 1910, 1, 279–286). Not only the gross measurements of height and weight but the development of the different parts of the body were recorded for three years and four months and six years and four months respectively. The value of such records for future studies of correlation of mental and physical growth is so evident that their multiplication is much to be desired.

Of a somewhat different character is the record by H. P. Swett (*Ped. Sem.*, 1910, 17, 104–110) of an imaginary companion so closely connected with a child's moral development as to be a sort of personified conscience. Her 'little girl' was the name given by the child to the imaginary playmate whose existence was first noted by the parents when the real little girl was about three years and eight months old. At the age of five, the visits of the little girl had become rare, though she still occasionally appears in times of moral conflict. As in most of the cases thus far recorded, the imaginary companion tends to disappear in proportion as replaced by real ones.

Among individual studies are also to be noted two accounts of unusually precocious children, Otto Pöhler (*Zsch. f. Kinderforschung*, 1910, 15, 166–171) and Christian Heineken (*Zsch. f. Kinderforschung*, 1910, 15, 225–229). The former before he had completed his second year was able to read German at sight, printed in either Latin or German letters, and not merely simple primer texts, but words of two or three syllables. When two years and two months old he was shown a medical calendar by a physician to whom he had been taken and asked to read. He read fluently "Re-ich's Medizinal Kalender. Begrundet von der Paul Börner." Though of a rather delicate build he has not been sickly and a year ago, at the age of eighteen, entered the University. During his gymnasium course his astonishing memory enabled him to gain distinction in languages and other subjects and he intends to specialize in German history during his university course. His physical measurements at the age of 1¾ and 16¾ years and his portrait at the same ages are included in Dr. Berkhan's article.

The very similar case of remarkable early development is Christian Heineken, who died in 1725 at the age of four and a record of whose life has been preserved in a book published by his teacher in 1779. His achievements in his fourth year were as follows: He could read German and Latin script and print but could not write, as his fingers were too weak. He could repeat the multiplication table in order and taken at random and could add, subtract, multiply and divide. In French he could repeat entire stories and in Latin 1,500 sentences from good authors. From his nurse he had learned *Plattdeutsch* and in geography had a fair knowledge of the principal places given in elementary maps. The child was exploited and caused great astonishment at an audience with Frederick IV.

In the line of speech development two records of vocabularies and one of a child's progress in definition are to be noted. Professor and Mrs. Whipple's record of 'The Vocabulary of a Three-year-old Boy With Some Interpretive Comments' (*Ped. Sem.*, 1909, 16, 1–22) is based upon the study of previous work and the methods which have proved most accurate, as is also Pelsma's record of 'A Child's Vocabulary and Its Development' (*Ped. Sem.*, 1910, 17, 328–369). The value of such careful individual records for the psychology of speech development is too obvious to need comment.

Professor and Mrs. Chamberlain's study of a child's ability to define (*Ped. Sem.*, 1909, 16, 64–103) is an extensive collection of verbatim material gathered from a child in the last two months of

the fourth year, and furnishes material for further study of the mental processes of the child mind.

Though not itself an individual study Dr. Partridge's *An Outline of Individual Study* (New York: Sturgis and Walton, 1910, pp. 245) is intended for the use of those interested in the study of children as individuals and also for teachers, physicians and students of education. Psychological, anthropological, biographical and other methods of study are outlined and simple comparative methods are described. The last section of the book contains a study of twin children.

Tests of Intelligence.—The effort to find a scale of measurement for general intelligence is by no means new among psychological problems, but a special impetus in this field has been given by the Binet and Simon tests, which were published in 1908 and translated into English by Dr. Goddard in the following year (*Training School*, 1910, **6**, 146–154). While their chief application has been in the grading of feeble-minded children according to the capacity of normal children, ranging from three to thirteen years, as indicated by the tests, they were originally made on normal children and have been tried on school children in this country. With all due allowance for their defects, and the various criticisms, of which perhaps the most complete is that by Decroly and Degand (*Arch. de psychol.*, 1910, **9**, 81–108) these tests have proved of great value and with modifications are in use in nearly all psychological clinics.

Though no other attempt has been made on so large a scale, there have been, since the appearance of the Binet and Simon tests, various contributions in this line, which offer suggestions as to possible methods. George Ries carried out a series of experiments with children (*Zsch. f. Psychol.*, 1910, **56**, 321–343) in which the tests used were paired words between which the child was required to find a connection, and the grading of intelligence rests upon the greater or less capability of different individuals to place a thought relation between two given concepts.

Another interesting test was carried out experimentally by Dr. F. Warburg (*Münch. med. Woch.*, 1909, **56**, 2511–2513) in the Volksschulen of Cologne on 1,270 children. This consisted of the ability to name the colors arranged in groups as follows: Group I., white, black, red, yellow, green, blue; Group II., brown gray, violet. A sharp distinction was made between the ability to recognize colors and the ability to name them, the test relating solely to the latter. The author found a surprising agreement between the results of

this test and the grading of pupils by teachers. While the particular percentages obtained have only a relative value, and the fact that girls usually show a higher average than boys can not be taken as a proof of superior intelligence since environmental influences must be considered here, the general results, reckoned separately for boys and girls, show that the ability to name colors increases with progress up the school grades and is in accord with the scale of intelligence. These results are confirmed by Dr. Cord's work (*Zsch. f. päd. Psychol.*, 1910, 11, 311–314) who, following Warburg's method, made further tests along this same line.

There have been a number of studies recently made in the anthropometrical field, correlating head measurements with intelligence, among which are Bayerthal (*Zsch. f. exp. Päd.*, 1910, 10, 197–218) and Büttner (*Die Gesundheitswarte*, 1910, 8, 28–34). Binet has contributed an extended study on the physical signs of intelligence (*Année psychol.*, 1910, 16, 1–30), not only head measurements of blind, deaf, and backward children being compared with those of normal development, but signs of degeneration such as irregular teeth, strabismus, malformation of the ear and palate, extreme facial asymmetry, nail biting and speech defects, etc., being taken into consideration. A study of the hands of both normal and abnormal children by means of photographs of both back and front was correlated with a study of the physiognomy by the same method, both profile and front view of the face being used.

Contents of Children's Minds.—During the year 1910 three studies on the contents of children's minds have been published, and that they come from widely separated portions of the globe adds to their value, since comparison should aid in ruling out the purely local. From St. Petersburg, we have the study of Dr. Netschajeff (*Zsch. f. päd. Psychol.*, 1910, 11, 306–310), the head of the paidological laboratory in that city; from Germany, the study of Hartmann (Frankfurt a. M.: Kesselring, 1910, pp. 248), and in the United States, the study made by Professor Libby of Northwestern University in coöperation with his students (*Ped. Sem.*, 1910, 17, 242–272). This latter presents some points of difference from other investigations on this subject, as children of different ages and social conditions were selected for study. The subjects were a five year old boy of good family, twelve children in an orphanage ranging in age from three to seven years, 408 first year high school pupils and 194 white and 125 colored children in the 4th, 5th, 6th, 7th, and 8th grades of a public school, the latter being divided into groups according to age, sex and color.

Hartmann's study is an extensive and careful analysis of children's knowledge on a wide variety of subjects, based on Herbart's doctrine of many sided interest and as the title indicates its motive is practical and pedagogical. Netschajeff's investigation was carried out with two groups of thirty children, each 5 to 7 years old, one of which attended kindergarten and the other did not. Twenty-five names of objects selected from animals, plants and natural phenomena were chosen as material for the test, the result of which was in favor of the kindergarten children.

Religious Development.—The discussion between Dr. Th. Hoepfner, of Eisenach, a physician of the psychoanalytic school, and Pastor Sperl, of Heilsbron, on 'Die Psyche des Kindes und der Gottesbegriff' can not fail to be of interest to all who are concerned with the religious education of children (*Zsch. f. Religionspyschol.*, 1910, 3, 350–360; 1910, 4, 174–177). Dr. Hoepfner, on psychological and therapeutic grounds, objects to the too early introduction of the child to the concept of God. His argument is briefly as follows. The concept of God is usually presented to children by the time they are three or four years old. That is, however, the time when the child is consciously gathering sense perceptions, especially optic ones, and tends to envisage all new ideas in visual terms and these are often inappropriate and fantastic. The false impressions thus acquired tend to persist as parts of the visual series and thus religious impressions fail to form an integral part of the social and ethical complex of ideas. Thus far, probably most students of child psychology would agree with the author, but does an acceptance of the facts necessarily lead to his conclusion that all religious instruction should be deferred until adolescence? With this conclusion Pastor Sperl takes issue (*Zsch. f. Religionspsychol.*, 1910, 4, 63–64) but he meets the question with arguments from authority instead of on the psychological basis, so that the two points of view have no common ground for comparison.

Another contribution, more to the religious pedagogy than psychology of childhood, though founded on the latter, is Edward Porter St. John's application of *Stories and Story Telling in Moral and Religious Education* (Boston: The Pilgrim Press, 1910), which is a timely little book containing many suggestions of practical value.

Reference should also be made here to chapters in two books already mentioned, namely Chapter XI. in Mrs. Mumford's book entitled The Dawn of Character and Chapter X., on religion, in Professor Sisson's book.

Emotions.—The last two years have not been especially productive in studies of emotion, but two or three contributions are to be noted. There are frequently appeals made to psychologists who are fortunate enough to be parents to keep individual records of the development of their children. Martin Buchner's study of *Die Entwicklung der Gemüthsbewegungen im ersten Lebensjahre* (Schul- und Universitätsgesch. 60. Giessen: Roth, 1909, pp. 19) is such a record and is a model of which many repetitions are to be desired, for in no realm of psychology is there more opportunity for study than in the field of the emotions. Though the list of emotions in the first year of life is a limited one, comprising chiefly feelings of pleasure and its opposite, surprise, curiosity, fear, anger, and some very elementary manifestations of æsthetic feeling and sympathy, careful observations at this stage can throw much light on the more complex developments of later years.

W. Boeck's *Das Mitleid bei Kindern* (Giessen: v. Münchow, 1909, pp. 114) is a questionnaire study based on 139 returns containing 630 cases, 348 boys and 281 girls. On the basis of these reports, the author concludes that elementary manifestations of pity occur during the first year and reach their height between the third and fifth year of life, and that after the twelfth year they retire into the background of consciousness. No comparison of this study with that of Saunders and Hall in the *American Journal of Psychology* for July, 1900, is possible because, though both are questionnaire studies, the latter made use of introspective and reminiscent material furnished by subjects from twelve years and upward in age, but the number of cases in Mr. Boeck's study seems somewhat too small to justify an age curve as to the manifestation of the emotion.

Queyrat's *La Curiosité* (Paris: Alcan, 1910, pp. 141) is a popular treatment of the subject with some pedagogic applications, especially in regard to malign and frivolous curiosity. Pathological forms of curiosity are also discussed and a chapter is devoted to its education. The treatment is however somewhat superficial, as curiosity is considered only as excited by novel impressions and there is no discussion of its intimate relation to interest. The book, however, abounds in good examples and the author's pedagogical and moral interest in the study are clearly recognizable.

Among the contributions to morbid phases of emotion are Varendonck's study of the phobias of children (*Rev. psychol.*, 1910, 3, 5–45) and Eulenberg's book on the suicide of children of school age (Leipzig: Teubner, 1909, pp. 30).

Child Welfare.—Among new developments in the field of child study are the various movements, which have centered in Clark University, relating to Child Welfare. Children have now for several decades been studied individually and collectively along psychological, physiological, neurological, and pathological lines, and a vast mass of material has been accumulated. But it is only within the last two years that a definite plan for the coördination and practical application of all this scattered material to questions of social psychology as affecting the child has been formulated. A general outline of the proposed work of the Children's Institute, some of which has already made some progress, is given in Dr. Hall's article (*Ped. Sem.*, 1910, 17, 160–165) and reports from the departments published in the *Pedagogical Seminary*. An account of 'The National Child Welfare Conference and its Relations to Child Study' may also be found in the pages of the same periodical (*Ped. Sem.*, 1910, 17, 497–504) as well as in the two volumes of the proceedings of the conference itself (New York: Stechert, 1910, pp. 273 and 292). The range of topics included in its discussions is so wide that it would be impossible to even enumerate them here, since they cover almost every subject directly or even indirectly connected with child welfare, from the educational value of dolls and story telling to the broad social questions involved in industrial insurance and eugenic movements. The main purpose of these movements is to make the connection between the results of scientific research and social and philanthropic work and reap the mutual benefit to be derived from such a correlation. On the one hand, the University has much of value in the results of child study which should be made accessible to social workers, and social workers can contribute much to the University by first hand reports of their special lines of work.

Among child welfare movements must be also mentioned the work of that unique national institution of Germany, the *Kaiserin Auguste Victoria-Haus*, founded in 1907 under the patronage of the *Kaiserin*. Its objects are to make scientific researches on all questions relating to infants and mothers, to collect statistics on infant mortality and all German organizations and institutions for the care and welfare of infants and also to collect such statistics from foreign countries and to make available by publication the scientific investigations carried on at the *Haus* itself. Of these, five have already been published under the general title *Ergebnisse der Säuglingfürsorge* (Leipzig und Wien: Deuticke, 1908–1910, Heft 1–5). While these publications deal primarily with the physical welfare of the child

and sociological questions, they often contain material of value to the psychologist. The subject of one of the published researches is the care of illegitimate children, and a part of this consists in a compilation of the laws affecting illegitimate children in the various countries of Europe, which have been collected and translated into German. The question of the moral responsibility of the physician in the case of infectious diseases, which is a serious question in connection with wet nurses, so extensively employed in Europe, is also discussed.

In this connection too must be mentioned an article of E. Gindes (*Arch. f. Kinderhk.*, 1910, **54**, 384–389) on the influence of dwellings upon the development of the child, a question to the importance of which the public is already showing signs of awakening.

Defectives.—Perhaps the most widely extended work in the field of child welfare is the work for backward, mentally and physically defective children. Here the literature is so voluminous and varied in character, ranging as it does from general treatises on the psychology and treatment of defectives to detailed descriptions of individual cases or analysis of a single type that only a few typical examples can be mentioned. The work of Shuttleworth and Potts on *Mentally Deficient Children, Their Treatment and Training* (London: H. K. Lewes; Philadelphia: Blakiston, 1910, pp. 249) is a standard work of unquestionable value to all teachers and students of mental defectives, though it by no means supersedes Tredgold's *Mental Deficiency* which appeared in 1908. In German, Martin Gluck's *Schwachbeanlagte Kinder* (Stuttgart: Enke, 1910, pp. 118) should also be mentioned.

Mention must also be made of Professor Shield's *The Making and Unmaking of a Dullard* (Washington: Catholic Educ. Press, 1909, pp. 296), a book of unusual psychological and human interest, for, in the words of the author, "its facts are not gathered from various sources to be moulded into one for the purposes of pedagogical exposition. The story, in its entirety, is a faithful transcript of the record that was burned into the heart and brain of the omadhaun and read by himself years after he had fought his way back to the company of normal and intellectual men." For, strange as it may seem, the man who now holds front rank among Catholic educators and took his degree in experimental psychology at an age not behind that of other men, was between the ages of eight and seventeen unable to read and had forgotten his previous knowledge of the multiplication table. Nor was this due to physical illness, for during these years

his health was excellent and he reached the full stature of manhood. Dr. Shields himself attributes the mental dullness to his rapid physical growth and lays stress on the phenomenon of alternating periods of mental and physical growth of which his own case was an extreme example. But one can not read the book without feeling that had psychological clinics been at hand in those days, the tragedy of the lost years between thirteen and seventeen might have been avoided, for to the psychologist in this field the signs of mental awakening would have been plain, long before their recognition was forced by the dullard himself. The book is written in dialogue form which, while it may add to the opportunity for insertion of pedagogical principles, detracts somewhat from the literary form, for the intense interest of the biographical portions tends to make one a little impatient of interruptions, however valuable they may be intrinsically. The most important recent literature, however, both in respect to psychological research, pedagogy and welfare of defective children is to be found in the periodicals devoted entirely to the subject, of which the most important are: *L'Enfance Anormale* (Bruxelles, founded 1906); *Eos* (Wien, founded 1904); *J. of Psycho-Asthenics* (Faribault, Minn., founded 1896); *Psychol. Clinic* (Philadelphia, founded 1907); *Training School* (Vineland, N. J., founded 1905); *Zsch. f. d. Beh. Schwachs.* (founded 1881); *Zsch. f. d. Erforschg. d. jugende. Schwachsinns* (Jena, founded 1907). Six psychological clinics have been established during the years 1909 and 1910. They are located as follows: Clark University (1909); Juvenile Psychopathic Institute, 4058 Clarendon Ave., Chicago (1909); State Institution for the Feeble Minded, Lincoln, Ills. (*J. of Educ. Psychol.*, 1910, 1, 435–444) (1909); University of Washington (1909); Woman's Medical College at Philadelphia (1910); Iowa Institution for the Feeble Minded, Glenwood, Ia. (1910). Of these, it will be noted that two are university departments, two are research departments in state institutions, one is connected with a medical school, and the Juvenile Psychopathic Institute of Chicago, supported by private philanthropy, is connected with the juvenile court. The rapidly growing appreciation of this line of child study may be seen from the fact that previous to 1909 there were but three such clinics in existence, Professor Lightner Witmer's at the University of Pennsylvania (*Ped. Sem.*, 1909, 16, 488–491), Dr. R. T. Wylie's at the State Institution for the Feeble Minded at Faribault, Minn., who was appointed psychologist in 1898, and Dr. Henry Goddard's at Vineland, N. J., since 1905. From the younger clinics, it is as yet too soon to expect much in the

way of published research, but the special studies published by Professor Witmer, Dr. Chase's account of the Clinic at Clark University (*Ped. Sem.*, 1910, 17, 189–203), Dr. Healy's article on 'The Mentally Defective and the Courts' and Dr. Goddard's contributions on heredity and mental tests (*Ped. Sem.*, 1910, 17, 387–397; *Amer. Breeders Mag.*, 1910, 1, 165–178) will serve to give an idea of the different lines of work and their practical applications. An article by Mary Sutton Macy points out the importance of the study of mental defectives in its relation to public school education (*J. of Educ. Psychol.*, 1910, 1, 132–144).

Perhaps no line of child study is likely to have more far reaching effects on child welfare than psychological investigations of speech defects, and of special interest here is the controversy carried on by Dr. Gutzmann of Berlin and Dr. Laubi of Zurich in the pages of the *Monatschrift für Sprachheilkunde* (1910, 20, 93–94, 97–117, 193–210, 241–247). Dr. Gutzmann maintains the position that speech defects are primary and the affective states accompanying them are secondary, bringing as proofs in support of his position that all stutterers without exception show serious defects in the mechanism of speech such as faulty breathing, etc., and also that, in some cases, psychic accompaniments may be entirely lacking. In this position Gutzmann agrees with Kussmaul's definition that stuttering is a coördination neurosis, founded on an original excitable weakness of the articulation apparatus. Dr. Laubi, who represents the Freudian school, maintains, on the other hand, that all speech defects are psychic in their origin and based upon an anxiety neurosis, not always the fear of the inability to speak plainly, but a fear that originates in a repressed conflict, of which the subject is unconscious, and which can be discovered and relieved only by psychoanalysis. In reading the discussion, one is struck by the fact that the Freudian cases are few in number and all of an hysterical type, a condition which is by no means always present.

A study of Dr. E. Bosworth McCready on the 'Relation of Stuttering to Amnesia' (*J. of Amer. Med. Assoc.*, 1910, 55, 208–9) is of interest as calling attention to the relation between the eadfness and speech defects.

Juvenile Faults and Delinquencies.—M. van Brabant's study of the psychology of the vices of children (Paris: Alcan, 1910, pp. 177) is a thoughtful and valuable contribution written from the point of view of child welfare, and recognizes that evils must be known in order to be remedied or prevented. The author begins with a study of

juvenile vice in antiquity, showing the conditions which existed in Egypt and Assyria, among the Medes and Persians, in Greece, Rome, etc. In face of many difficulties the author has succeeded in collecting more or less complete statistics in regard to 18,499 children as to the prevalence of the following vices among them: self abuse, stealing, lying, smoking, frequenting cafés, and quarrelsomeness. The results, admittedly incomplete, since information on some points was very difficult to obtain, show 68.37 per cent. of the boys and 45.2 per cent. of the girls as guilty of one or more of these forms of vice. Stealing and lying are somewhat more frequent among the girls, smoking is exclusively a boy's vice and quarrelsomeness as expressed by fighting is more common among them. Lying is by far the most frequent vice for both sexes, 28.45 per cent. and 29.3 per cent. of the girls being either intermittent or inveterate liars. Among the causes tending to bring about these conditions, the author discusses hereditary defects, prematurity with its accompanying unstable nervous system, alcoholism of parents, bad family conditions, loss of one or both parents or desertion by them, divorce of parents, illegitimacy, bad housing conditions, the seductions of luxury, and onanism due to bad physical conditions or heredity. From the causes, the author proceeds to the question of prophylaxis which in the broadest sense must be based on improved social conditions. Some of the possible remedies suggested are an extension of school supervision, organization of protective classes for children, more homes and institutions for defectives, and a thorough revision of the laws dealing with the responsibility of parents.

The address of Mr. Comstock delivered at the Child Welfare Conference held at Clark University in 1909 (*Ped. Sem.*, 1910, **17**, 403–420) not only made a deep impression upon those who heard it, but in its printed form should be read by every educator and parent. It is a brief account of a society to which Mr. Comstock has devoted the work of a lifetime and whose object is to keep the minds of children from contamination by impure literature, pictures, etc. He has destroyed literally tons of indecent literature and pictures, and the plates from which they were manufactured, and has carried on a crusade against the manufacturers and purveyors by prosecution, by successful efforts to obtain better laws, and by international coöperation, until the production and circulation of such matter is noticeably decreasing, though the time has not yet come when the world can spare Mr. Comstock.

It would be difficult to find one better qualified than Judge

Lindsey (*Proc. and Addr. Natnl. Educ. Ass.*, 1909, 47, 146–157) to discuss the delinquencies of children. His intimate personal acquaintance with youthful lawbreakers has given him both the worst and best sides of the situation. While he confirms by his opinion the figures of M. van Brabant in the words, "I am inclined to think that if we tried the morality of children in the public schools of this country by this test—namely: is the child disobedient, does he swear, does he lie, does he steal, is he impure in word, thought, or act—the results would be so startling that I have always seriously doubted the wisdom of its public discussion except in a most guarded and careful way," he nevertheless takes a hopeful view of the situation, since in spite of such sins many a man has become a good citizen. Judge Lindsey would place the responsibility for the development of strength of character, which is the only real protection against immorality, on the home, the church, the school, the neighborhood, to which in some cases he would add the state as a substitute for the parent. He brings a strong indictment against public officials who tolerate lawlessness, the crimes of industrial wealth, the school and the home which have failed to give right standards and strengthen the character of the child to resist temptation. Among the serious causes of juvenile delinquency he places the bad example of business men and public officials and emphasizes personal influence as the most powerful factor in moral or immoral evolution. Physical conditions also have an important relation to moral development, and Judge Lindsey does not consider it far fetched to point out bad teeth as one of the causes of immorality, nor does he leave out of consideration the importance of industrial training as a factor in moral efficiency.

Along the line of the intimate connection between physical defects and moral degeneracy are to be noted several articles in the *Psychological Clinic*, especially Professor Witmer's contributions entitled 'The Restoration of Children of the Slums' (1910, 3, 266–286) and 'Criminals in the Making' (1911, 4, 221–238) and the articles dealing with bad dental conditions, one a discussion by Dr. Holmes on the question 'Can Impacted Teeth Cause Moral Delinquency' (1910, 4, 19–22) and a further contribution by Dr. Upson on 'Moral Aberration Due to Physical Irritants' (1910, 4, 149–154). Mention should also be made of a contribution by A. MacDonald to development of faults in children at the age of puberty and associated with physical defects and abnormalities (*Jahrb. f. Kinderhk.*, 1910, 71, 180–188).

In one of the journals devoted especially to study of defectives there is a contribution by Professor Vogt to the psychology of child-

ren's lies (*Zsch. f. d. Erforschg. d. jugendl. Schwachsinns*, 1910, 3, 394–438) chiefly as these appear in the mentally defective. Among the points brought out is the greater prevalence of lying among girls and difference in character of the lies of girls from those of boys. The pathological lies of boys, the author finds, have more of a defensive character, while a fantastic autosuggestibility is more characteristic of those of girls. Puberty is the most marked age for increase of lying, and many of the most extreme cases are to be found among children who are really feeble-minded or hysterical degenerates, though by no means have all cases a hysterical character.

The most important contribution to the psychology of children's lies withing the last two years is the painstaking work of Professor and Mrs. Stern (Leipzig: Barth, 1909, pp. 170) for, though a comparatively small portion of the book is devoted to lying in the sense of moral delinquency, the psychological development of the child in ability to remember and reproduce correctly, the falsifications which arise normally from defective memory or vivid imagination, the tendency to falsehood which may arise from neglect of training and recognition of these factors and the practical applications are so clearly presented that a complete translation of the book for the benefit of those who do not read German is much to be desired. A résumé of portions of the work is to be found in the translated report of Prof. Stern's lectures given at the Clark University Conference in 1909 (*Amer. J. of Psychol.*, 1910, 21, 270–282).

Another interesting contribution to this subject is the monograph of Kuhn-Kelly entitled *Lüge und Ohrfeige* (*Beiträge zur Kinderforschung u. Heilerziehung.* Langensalza: Beyer, 1910, pp. 23). Though pedagogical in its import, its plea for a personal and sympathetic treatment of the child who lies is based on psychological principles.

Mr. Acher's study of the primitive activities of children and their analogies to the activities of primitive man (*Amer. J. of Psychol.*, 1910, 21, 114–150), while not falling under any of the above classifications, is a questionnaire study which brings out some interesting points. In children's play with blocks, sand and earth, the snow and ice, strings, collection of stones, their use of points and edges, propensity for striking or throwing, attempts to modify their own faces or figures and their attitude toward clothing, the influence of imitation, natural interests, and their resemblance to activities of primitive man, their meaning in the development of the child are all studied from the genetic point of view and form an exceedingly

suggestive contribution to genetic psychology. The rapid spread in the last few years of the Freudian school and the method of psychoanalysis demands recognition in its relation to child study, although this is chiefly in the pathological field and it may be doubted whether the contribution to the psychology of normal childhood amounts to more than a calling of the attention to the fact that the possible development of sex life in young children can not be ignored and that both careful watching and training are necessary from even babyhood. It is probable that such cases as that of little Hans described by Freud (Leipzig and Wien: Deuticke, 1909, pp. 109; *Psychiat.-neur. Woch.*, 1908, **9**, 465–467) or the two cases described by Jung in his lectures given at the Conference held at Clark University in the fall of 1909 (*Amer. J. of Psychol.*, 1910, **21**, 219–269; *Jahrb. f. psychoanal. u. psychopath. Forsch.*, 1910, **2**, 33–58) are of rare occurrence among normal children.

ADOLESCENCE

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Studies in adolescence during the past two decades have orientated essentially from the biological and sociological points of view. Many problems and broad generalizations have been evoked but there is little definite information on the mental traits of adolescents in regard to perception, reasoning, attention, association, memory, and voluntary activities, and little knowledge of the learning processes involved in high school subjects of instruction. Experimental psychologists have in the past seldom frequented our high schools and it has been assumed that studies on normal adults or infants would reveal the mental characteristics of a rapidly developing adolescent mind. Emphasis has also been placed on scattered analogies based on the individual's recapitulation of phylogenetic development.

A survey of the literature and tendencies since 1909 indicates that we are on the threshold of a second epoch in the study of adolescence, which promises to be characterized by empirical scientific studies and classroom experiments on the mental and physical development of individual boys and girls for the consecutive years from ten to eighteen. The contributions have centered, in the main, around five sources with a shifting toward the psychological point of view. (1) A few books of encyclopedic scope have appeared.

(2) The high school, especially the 'experimental high school,' has become a working laboratory for university departments of psychology and education. Accompanying this tendency has been a strong impetus for secondary school teachers to seek professional training, which is leading to the investigation of how adolescents learn and the explanation of how the methods and materials of instruction are best fitted for their particular needs. (3) A pronounced interest in promotion and grading, retardation and elimination of pupils has arisen. (4) There has been developing an intensive study of juvenile delinquency and special schools for this period. (5) Several contemporary tendencies show that the general reader as well as the special student is concerned with sex hygiene. This *Sammelbericht* aims to review these five sources.

General Treatises.—The most noteworthy book of this year is by G. Stanley Hall, *Educational Problems* (N. Y.: Appleton, pp. 725; 720) which, although containing repetitions from his *Adolescence* (and from chapter to chapter), gives many valuable results and conclusions from the vast field attacked by him and his numerous collaborators. The two volumes summarize the author's Saturday morning lectures for the past twenty-five years. Chapters of particular import in connection with adolescence are: Religious Training of Children (IV.); Moral Education (V.), which stresses discipline and the pedagogy of juvenile crime; Children's Lies (VI.), their causes, character and remedies; Pedagogy of Sex (VII.), its anthropological significance and methods of instruction; The Budding Girl (IX.), which, to the present writer, is least satisfactory; Special Child Welfare Agencies Outside of the School (XI.); Preventative and Constructive Movements (XII.), as boys' clubs, charitable organizations, playground associations, and open air schools; The Psychology and Pedagogy of Special Subjects in the High School, with many suggestions; Some Defects of Our Public Schools, including a discussion of the dangers of coeducation, defects of promotion, inferior methods of teaching, elimination, the ideal school (XXII.); and The American High School (XXIII.), which emphasizes the need of extending the high school downward to the sixth grade.

The book is very extensive in its scope, containing discussions of educational movements and experiments, summaries and conclusions, and a great deal of auxiliary material bearing indirectly on the general problems. It is a worthy companion to the author's *Adolescence*, and we shall look forward with interest to his promised treatise on psychology.[1]

[1] See special review on p. 363.

The monograph which appeared early in 1909 by G. Compayré, *L'adolescence: studies de psychologie et de pédagogie* (Paris: Alcan, 1909, pp. 195), is practically based on Hall's *Adolescence*, treating in general the same topics. A little book from England, *The Adolescent*, by J. W. Slaughter (London: Swann Sonnenschein, 1911, pp. 115), gives a brief clear outline picture of the period. Serving as an introduction to Hall's larger work it differs in several particulars and offers us, as Findlay writes in the Introduction, "A cooler diagnosis which leads us to tremble less for the future of our race." There are ten chapters, no new experiments are cited, no references given, and the book emphasizes that "The central fact of adolescence is emotional change." From the standpoint of experimental psychology a treatise by A. Lemaitre, *La vie mentale de l'adolescent et ses anomalies* (Saint-Blaise: Foyer Solidariste, 1910, pp. 240), is a valuable accession which brings together into more usable form Professor Lemaitre's studies on the psychology of adolescence that have been appearing in various psychological journals. He maintains we should not search for a single type of adolescent mind, for "ce type moyen est une pure convention, qui ne correspond à aucun sujet pris insolément." The book is suggestive, comprehensive within its field, and technical. It treats, among other topics, adolescent thinking, internal speech, various forms of synopsias—such as the tendency to personify letters and figures, color hearing, number forms, multiple personality—with some good examples of autoscopy, and a suggestive chapter for the teacher on good and bad mental habits. Another excellent treatment containing much of psychological import on account of the keen analysis of the adolescent mind was published in 1909 by P. Mendousse, *L'âme de l'adolescent* (Paris: Alcan, pp. v + 306). This book is worth careful reading and should be made accessible to those who read only English. The author divides the field into three parts, which deal respectively with the early signs of adolescence and the rites and customs associated historically with its advent, the psychophysical effects accompanying growth, psychic changes accompanying the initiation of puberty and the development and perversions of sex instincts. The new mental traits, feelings, and characteristic instincts evoked at puberty are then carefully analyzed, especially the phenomena dealing with love, bashfulness, esthetic feeling, reason, and courage. The last portion of the book deals with conflicts, incoherency, instability of mental activities, and the incoördination of physical growth and movements, which lead to a study of fatigue. From an educational

standpoint the adolescent is in a transition stage between childhood and maturity, and should be subject to careful discipline. The author approaches his subject from the modern genetic point of view.

Useful references bearing on adolescent development will be found throughout the pages of F. E. Bolton's *Principles of Education* (N. Y.: Scribners, 1910, pp. 790), which is the most comprehensive treatise so far published in the field which the title connotes; also in *L'éducation des anormaux* by J. Philippe et G. Paul Boncour (Paris: Alcan, 1910, pp. 212); and in E. Formiggini-Santamaria's *La psicologia del fanciullo normale ed anormale con speciale riguardo alla educazione* (Modene: Formiggini, 1910, pp. 342).

A few magazine articles and pamphlets are deserving of special mention. M. E. Sadler, 'Education during Adolescence' (*Nature*, **82**, 325–327), characterizes the English system of education for boys and girls as a torso. "The years from thirteen to sixteen are years of educational leakage and we must look forward to some form of compulsory attendance for all persons between fourteen and seventeen years of age." The article is suggestive from the educational point of view. W. Brown of the British Psychological Society, 'Educational Psychology in Secondary Schools and Notes on a Qualitative Analysis of Mathematical Intelligence' (*J. of Phil., Psychol., etc.*, **7**, 14–18, 526–528), aims to bring experimental psychology into direct connection with the English secondary schools. Hans Gudden, *Pubertät und Schule* (München: O. Gmelin, 1911, pp. 31), gives an outline of mental life during puberty and sex education, emphasizing pedagogical consequences in the school and home. W. Paetz, 'Pubertät und Schule' (*Zsch. f. Päd. Psychol.*, **11**, 453–462), discusses, without experimental data, practical suggestions on the periods of adolescence, advocating a limited amount of instruction, the need of shifting the course of study, and the wise use of play. A. Cramer, in his *Pubertät und Schule* (Leipzig: Teubner, 1910, pp. 16), has made a brief survey of adolescence by calling attention to the importance of the period, by emphasizing its relation to the school, and by giving a comprehensive view of the anatomical, physiological, and psychophysical problems involved. The schools, he claims, are not for sick or psychopathic individuals but for normal children, and teachers should know more about the pubescent period in order to recognize abnormal conditions. This is particularly true in regard to sexual tendencies. R. Schneider published a usable little treatise, *Pubertät und Auge* (München: O. Gmelin,

1911, pp. 17), on eye diseases and defects during adolescence, treating of styes, various kinds of inflammations, functional disorders accompanying nervous debility and the causes and hygiene of myopia. Among the most important French contributions to be cited here are those by J. Varendonck, 'Phobies d'enfants' (*Rev. Psychol.*, 1910, 3, 5–45); Berillon, 'L'anorexie des adolescents: particularités mentales et traitement psychologique' (*Rev. de l'hypnot.*, 23, 40–49); A. Marro, *Evolution psychologique humaine à l'époque pubère* (Traité int. de psychol. path., Paris: Alcan, 1910, pp. 708–814).

A significant statement on coeducation has been made by L. Burgerstein, 'Coeducation' (*Ped. Sem.*, 17, 1–15), who, after calling attention to the differences between boys and girls, concludes, in agreement with Burnham in this country, that "With regard to the mental gifts and abilities of girls there are hardly any serious reasons against coeducation in high schools, but that their physical state and development speak decidedly against loading them with such a burden as the boys have to bear now in our European high schools."

Experimental High Schools, Special Methods Courses, and Experimental Research.—Forty-two universities and colleges in this country now offer practice courses for secondary school teachers, and nine universities and one college have experimental high schools under their immediate direction and control. This is beginning to furnish one of the most fertile fields available for the accumulation of data on the psychology and pedagogy of adolescence. See monograph on *Observation and Practice Teaching* by Farrington, Strayer and Jacobs (Univ. of Chicago Press, 1909, pp. 80); also, B. T. Baldwin, 'Practice Schools in University Departments of Education' (*J. of Educ. Psychol.*, Oct., 1911).

Courses on adolescence are also given in several universities with a marked tendency toward the psychology of high school subjects; for example, Chicago (Judd), Clark (Hall), Iowa (Bolton), Nebraska (Luckey), Pennsylvania (Holmes), Texas (Baldwin). A good start in experimental methods of teaching high school subjects has been made by the *Journal of Educational Psychology*, whose editors have arranged for a series of twenty-two articles on problems dealing with the common school branches of learning and Latin, German, English, physics, biology, chemistry, botany, algebra, geometry, and sex hygiene. Mention should be made of a similar movement by the *Zeitschrift für pädagogische Psychologie und experimentelle Pädagogik*, Leipzig.

It is from genetic studies of a comparative nature that detailed

psychological data on the adolescent mind are to be obtained rather than from the general anthropological and sociological material which has been our chief source and which often lacked experimental evidence. The greatest need in the present study of adolescence is for detailed intensive individual studies of boys and girls for consecutive years throughout the period from ten to eighteen based on psychological tests and educational experiments. We have no such studies, but we are beginning to see the necessity of this type of work.

C. Burt's 'Experimental Tests of General Intelligence' (*Brit. J. of Psychol.*, 3, 94–177), is an important recent investigation. He selected common psychological experiments and attempted to standardize them and apply them to two groups of adolescent boys. The tests consisted of five series of twelve groups on the processes of peripheral discrimination, simple reaction, reaction with discrimination and with recognition, immediate memory, a trial and error method of forming associations during motor activity, an apperceptive approach to the formation of association during perceptual activity and maximal effort of sustained attention. One of the three aims was to determine, "Can the presence of general intelligence be detected and its amounts measured?" The sensory-motor tests proved of least significance but when the others were arranged according to the results there was a list showing correlations with the headmasters' order to the extent of .85 at one school and .91 at the other. The tests, according to the experimenter, give a means of ranking intelligence more accurately than examinations and probably a better means than the teacher's estimation.

Another type of careful individual work has been done by F. G. Bonser, *The Reasoning Ability of Children of the Fourth, Fifth and Sixth School Grades* (Columbia Univ., 1910, pp. 141). Three hundred and eighty-five boys and three hundred and seventy-two girls were given mental tests. Many conclusions are worked out, some of the more general ones being: there are different correlations for boys than for girls, a higher correlation for boys than for girls, rhythms of increase in ability with age differing with boys and girls.

W. H. Winch, 'Some Measurements of Mental Fatigue in Adolescent Pupils in Evening Schools' (*J. of Educ. Psychol.*, 1, 13–23, 83–100), has worked out a practical method for testing fatigue and concludes that the evening schools are not likely to prove profitable for adolescent students. F. Reinhold, 'Beiträge zur Assoziationslehre auf Grund von Massenversuchen' (*Zsch. f. Psychol.*, 54, 183–214), found where stimuli words were given to evoke associations

for thirty girls for each of ten successive grades, ranging from seven to seventeen, that, among other things, younger children showed greater individual differences than adolescents. W. Libby (and five others) worked with four hundred and eight first year high school pupils on 'Contents of Children's Minds' (*Ped. Sem.*, **17**, 242–272).

A very interesting report was made by W. L. Foster, 'Physiological Age as a Basis for Classification of Pupils Entering High School and Relation of Pubescence to Height' (*Psychol. Clinic*, **4**, 83–88), which seems to prove that adolescents classified according to physiological age show a higher percentage of promotions and a lower percentage of discharges than those rated by the usual grade classification; and that grouping by height, on account of height correlation with advent of puberty, produces about the same results as grouping by stage of pubescence. B. T. Baldwin's investigation, 'Individual Differences in the Correlation of Physical Growth of Elementary and High School Pupils' (*J. of Educ. Psychol.*, **2**, 150–152), has been extended to five hundred boys and five hundred girls for growth in height, weight and lung capacity, including two hundred and twenty-five individual curves for consecutive measurements of yearly and half-yearly increments from five to twelve years and for the ages from four to eighteen. The results show marked individual differences, and prove that a composite curve of average measurements from different groups of individuals cannot give an accurate conception of growth, since the characteristics of different types of the same chronological age but different physiological ages tend to obliterate each other. (To be published by the U. S. Bureau of Education.)

Mention may be made of a few of the educational experiments that are being tried out with incorrigible and defective boys who have been segregated for short periods of time for individual and collective study in order to correct abnormalities, determine physical defects, awaken worthy interests or develop a wholesome point of view. A significant type of experiment was conducted last summer by A. Holmes and N. C. Cameron, 'An Educational Experiment with Troublesome Adolescent Boys' (*Psychol. Clinic*, **4**, 155–178). Another study, accompanied by the publication of twenty individual charts, was worked out at Girard College by R. L. Johnson, 'A Score of Difficult Boys' (*Psychol. Clinic*, **5**, 121–127); and another by M. B. Greene, 'A Class of Backward and Defective Children' (*Psychol. Clinic*, **3**, 125–133).

The year 1910 gave us an important book, *Applied Ideals in Work with Boys* (New York: Y. M. C. A. Press, pp. 256), on the physio-

logical grouping of adolescent boys, adolescent psychology, self government, sex instruction, the boy scouts, and similar topics by C. W. Crampton, W. S. Hall and nine others. Some other books dealing with social instincts of adolescents and remedial in effect are: W. B. Forbush and F. L. Masseck, *The Boy's Round Table* (Potsdam, N. Y.: F. L. Masseck, 1909), which gives the history and purposes of the organization; H. Payson, *Boy Scouts* (New York: Hurst, 1910); and E. S. Thompson and R. Baden-Bowell, *The Boy Scouts of America* (New York: Doubleday, Page & Co., 1910). No less than one hundred and fifty clubs for street boys were recently catalogued by W. B. Forbush, 'Boys' Clubs' (*Ped. Sem.*, **16**, 337–343).

Promotion and Grading, Retardation and Elimination.—Laboratories for experimental education are investigating various aspects of the problem of adolescent development through a comparison of elementary and high school grades, and of high school and university grades. The most significant contribution in this field, which will help materially to standardize educational experiments, has been made by W. F. Dearborn, 'The Relative Standing of Pupils in the High School and in the University' (*Bull. Univ. Wis.*, 312, H. S. Series no. 6, pp. 44). A similar study was made at the University of Iowa by W. R. Miles, 'A Comparison of Elementary and High School Grades' (*Ped. Sem.*, **17**, 429–450). Two other studies have been published in the *Elementary School Teacher* by F. W. Johnson (**11**, 63–78) and W. S. Smiley (**11**, 249–265). Attention should also be directed in this connection to C. H. Judd's 'Scientific Study of High School Problems with Special Reference to Types of Instruction and Individual Differences' (*Sch. Rev.*, **18**, 84–98.)

Closely related to the above is the study of retardation, which is essentially a psychological problem, concerning children of the early adolescent age and their characteristic tendencies. Since the pioneer work of Thorndike and Ayres the *Psychological Clinic* has contributed noteworthy articles by L. Ayres (**4**, 189–192); R. P. Falkner (**4**, 213–220); G. W. Gayler (**4**, 40–45, 79–82); Jones (**5**, 63–96, 99–120); F. E. Lurton (**5**, 13–19); C. A. A. J. Miller, (**3**, 136–140); C. R. Squire (**4**, 46–53); E. L. Thorndike (**3**, 232–243, 255–265); A. E. Wagner (**3**, 164–173); L. Witmer (**4**, 121–131). Also, in the *Educational Review*, L. B. Blau (**40**, 51–64); R. P. Falkner (**38**, 122–131); W. D. Sheldon (**18**, 121–131); *Elementary Teacher*, W. F. Dearborn (**10**, 1–13); *Journal of Educational Psychology*, J. E. W. Wallin (**1**, 445–466); *School Review*, G. R. Johnson (**18**, 680–694).

The psychology and pedagogy of the gifted child is also claiming both popular and scientific attention. W. Stern has given in *Das übernormale Kind* (Der Säemann, 1910, 67–72, 160–167), an outline of experimental pedagogy for gifted children, and in the city of Baltimore a special high school which permits eighth-grade pupils to complete their high school course in one, two, or three years, has been organized with some striking results. See J. H. Van Sickle, 'Provisions for Gifted Children' (*El. Teacher*, 10, 357–366). Several precocious adolescents have been discussed in popular magazines, and a comparison has been made between the son of Dr. Boris Sidis and J. S. Mill by T. A. Williams (*Ped. Sem.*, 18, 85–103). All writers emphasize good training as the essential element.

The preceding article of this number of the BULLETIN has discussed the organization of psychological clinics and recent contributions to the psychology of mental deficiency. One of the most aggressive movements in educational psychology in this country and in Europe is to be found in the attempts to test, verify, modify, or supplement the Binet and Simon and the de Sanctis tests in their application to normal, subnormal, and mentally deficient children. The most recent contribution is by H. H. Goddard, 'Two Thousand Normal Children Tested by the Binet Measuring Scale of Intelligence' (*Ped. Sem.*, 18, 232–259). These tests, especially when individual records are given in detail, are throwing much light on the mental traits of adolescents. For a review of the literature of the field for 1910 see 'Tests,' F. N. Freeman (PSYCHOLOGICAL BULLETIN, 8, 21–24).

Juvenile Delinquency and Special Schools.—Laws have been passed in twenty-three states in this country, which tend to regard the adolescent offender as a delinquent instead of a criminal, *i. e.*, as one who should be placed in a detention home or on probation instead of in a prison or a reformatory. For a general survey of the laws see T. J. Homer, *Juvenile Court Laws of the United States* (New York: Russell Sage Foundation, 1910, pp. 150). In his *Preventive Treatment of Neglected Children* (New York: Charities Pub. Comm., 1910, pp. 420) H. H. Hart gives some very good chapters on juvenile reformatories, societies in the interest of children, the work of the juvenile court, and the study of neglected children. In 1909 Germany gave us an important monograph by P. Pollitz, *Die Psychologie des Verbrechers: Kriminalpsychologie* (Leipzig: Teubner, pp. 148), which shows in graphic form, after a study of one hundred thousand criminals of different ages, that there is a remarkable acceleration in the

increase of crime from twelve to sixteen, then a decline, and another rise from thirty to forty. The book is condensed, comprehensive, and practical. The same year France gave us one of our most reliable studies by G. L. Duprat, *La criminalité dans l'adolescence* (Paris: Alcan, pp. 260). This writer finds the causes of delinquency in the individual's desire for freedom from restraint on the one hand and the unwholesome stimuli furnished by society on the other. The remedies lie in special schools, compulsory school attendance, and a higher standard of public opinion. The book discusses juvenile crime and criminal types of adolescents, the sociological and hereditary causes, defective education, immoral influences, economic conditions, social dissolution, the remedies, repression and correction, and the need of public vigilance, private philanthropy and preventive education. America contributed a good treatment on juvenile delinquency intended for the reading public by G. B. Mangold, *Child Problems* (New York: Macmillan, pp. 391); an admirable book by Jane Addams on *The Spirit of Youth in the City Streets* (New York: Macmillan, pp. 162), and in 1910 W. R. George gave us the history and ideals of the *Junior Republic* (New York: Appleton, pp. xvi+325). For additional material on 'Juvenile Delinquents' and 'Moral Education' see the preceding paper. In connection with the latter topic the writer will mention *The Psychology of Religious Experience* by E. S. Ames (New York: Houghton Mifflin Co., 1910, pp. 440). This book is excellent and contains a good chapter on Religion and Adolescence.

A wealth of material is developing in connection with the juvenile probation courts, where the psychologist and teacher act in conjunction with the judge. As a pioneer in this field Dr. W. Healy may be cited. At the beginning of the year 1909 he organized the Juvenile Psychopathic Institute of Chicago. While the results of the work have not been published, an important contribution to the problems of adolescence is expected, since an intensive and consecutive study is being carried out on a limited number of delinquent 'repeaters,' supplemented by a thoroughgoing method of 'following up' cases. The work falls under the heads, sociological, medical, and psychological. See W. Healy, *Report of Committee A, American Institute of Criminal Law and Criminology* (N. W. Univ. Law Pub. Assoc., 1909, pp. 14); F. S. Churchill and J. A. Britton, 'Medical Work of the Juvenile Court of Cook County, Chicago' (*Archives of Pediatrics*, 1910). At present stress is being laid on Freud's methods of psychoanalysis, the application of the Binet and Simon tests, and

the Institute's special system of tests which include the *Aussage* tests, verbal and visual-verbal memory, memory for geometrical figures, two cross line tests, code test, four types of form board test, puzzle box, construction box, learning test, information sheet, work in arithmetic, reading, and writing. See W. Healy and G. M. Fernald, *Tests for Practical Mental Classification* (PSYCHOL. REV. MON., 13, Whole No. 54, pp. viii + 53). Two studies in this connection are 'Criminals in the Making' by L. Witmer (*Psychol. Clinic*, 4, 221–238) and K. Kruppa's 'Flegeljahre und Pubertätszeit als Ursachen der Kriminalität Jugendlicher' (*Zsch. f. Kinderforsch.*, 14, 197–207, 225–235).

Sex Hygiene and Its Instruction.—The writings on sex hygiene have been prolific during the past two years, greatest attention being paid to the subject in Germany and America. The books may be grouped into those prepared for adolescents, those for parents and teachers, and those of a technical and medical nature. Several, at places, are saturated with the taint of morbid confessions and exploit abnormal conditions to such an extent as to arouse fear, if not disgust, in the minds of the readers.

The writer has found no book that satisfactorily fulfils conditions for young boy and girl readers, but among the most reliable may be mentioned those by W. L. Howard, *Confidential Chats with Boys* (pp. 172), *Confidential Chats with Girls* (pp. 135) (New York: E. J. Clode, 1911). These are sane, practical, scientific, and probably as safe as any written. The latter is the better book. E. B. Lowry's *Confidences* (Chicago: Forbes & Co., 1910, pp. 94) is adapted for very young girls, and the same author's *Truths* (Chicago: Forbes & Co., 1910, pp. 95) and W. S. Hall's *From Youth to Manhood* (New York: pp. 106) for adolescent boys.

Three manuals bearing directly on the pedagogy of sex instruction are: P. Zenner's *Education in Sexual Physiology and Hygiene* (Cincinnati: Robt. Clark Co., 1910), a series of talks printed by the Board of Education of Cincinnati and distributed among principals and teachers; *Genesis: A Manual for the Instruction of Children in Matters Sexual* by B. S. Talmey (New York: Practitioners' Pub. Co., 1910, pp. 194), containing 47 drawings, 19 cuts and many suggestions and model lessons for children from four to eighteen; and P. Groebel's little book on *Sexualpädagogik in den Oberclassen höherer Lehranstalten* (Hamburg: Voss, 1909, pp. 88). For a general summary and other references see C. Schmitt's 'The Teaching of Facts of Sex in the Public School' (*Ped. Sem.*, 17, 229–241), and *Sexual Problems*, for 1909–11.

C. R. Henderson's *Education with Reference to Sex* (Univ. of Chicago Press, 1909, pp. 163) is a detailed and technical treatment of the subject, written from the pathological and social points of view, for adults with scientific training but not for boys and girls. The book is published by the National Society for the Study of Education. Another exhaustive scientific treatment may be found in *The Sexual Life of Woman*, E. H. Kisch (New York: Rebman Co., 1910, pp. 698). This book contains elaborate tables and treats the adolescent girl from the standpoint of the hygiene of maturity and the accompanying anatomical changes, diseases of various organs and functional and habitual disorders. Two other notable books written primarily from the physiological and sociological points of view are *Sex in Relation to Society*, by H. Ellis (Studies in the Psychol. of Sex, **6**, Phila.: Davis, 1910, pp. 656), and *Das sexuelle Problem und seine moderne Krise*, by W. E. Mertens (München: Kupferschmid, 1910, pp. 474). The former contains a very good chapter on adolescence and sexual education; the latter deals with the problems of sexual hygiene, disease, education, and advocates more instruction for children. A. Moll's treatise, *Das Sexualleben des Kindes* (Berlin: Walte, 1909, pp. 313) is well known, having appeared early in 1909; it contains material on the organs and instincts of sex, and sex differences in childhood.

Freud and his followers have contributed most to our knowledge of the psychological analysis of sexual development through their methods of psychoanalysis which trace to their source many processes that at first do not appear connected with the sex instinct. The most important contributions bearing directly on this subject are: S. Freud, *Jahrbuch für psychoanalytische und psychopathologische Forschungen*, 1909, 1; 'Freud's Psychology' by E. Jones (PSYCHOLOGICAL BULLETIN, 7, 109–128); 'The Origin and Development of Psycho-analysis,' by Freud (*Amer. J. of Psychol.*, **21**, 181–200); 'The Association Method,' by C. G. Jung (*Amer. J. of Psychol.*, **21**, 219–269), and 'Psycho-analysis and Education' by E. Jones (*J. of Educ. Psychol.*, 1, 497–520). The aims of enlightenment should be, says the last writer, "To inculcate an attitude of purity and naturalness toward matters of sex, . . . to prepare for the physiological occurrences inseparable from his existence . . . and to give him such knowledge of sex hygiene as to enable him to guide his sexual life amidst the numerous dangers and difficulties that are likely to beset it."

SPECIAL REVIEWS

SOME RECENT BOOKS ON EDUCATIONAL PSYCHOLOGY

BY WALTER R. MILES
University of Iowa

Educational Problems. G. STANLEY HALL. New York: D. Appleton and Company, 1911. Pp., Vol. I., xiii+710, Vol. II., 714. $7.50 net.

This monumental work is the sequel to the author's *Adolescence.* Many of the theories presented there, seven years ago, are here brought to bear upon specific problems of American education. A glance at the table of contents and sub-headings reveals a wealth of material both in the range of problems presented and in the many-sidedness of the treatment. It is a source-book on conservation and development in education from the point of view of the Clark School in psychology and education. The reviewer does not know of a work more calculated to create educational faith and enthusiasm than these mature opinions from President Hall. The reviewer can do no better than try to present a few of the leading thoughts from each chapter.

Introduction: Pedagogy has gradually become established as a full chair in universities and colleges, we have a seemingly very large educational budget and there are other points of advance, but our schools are not meeting our needs. The reader is called upon to decide which condition we approach more nearly, the ideal or the worst type of school. "Our destiny is at stake. America today needs a new educational dispensation. Our system is not fulfilling the purpose for which our fathers established it, nor is it molding men as it did in older days when it was simpler and cheaper, and in these volumes I have tried to point out in some detail, as best I could, why, as well as to suggest the needed cures as I see them."

The Pedagogy of the Kindergarten: "Oh, for one really ideal kindergarten in the world to demonstrate 'what education can be and do' to carping and incredulous pedagogues with their distorted and mean idea of children, and their dense ignorance of their deeper nature and their possibilities!" The kindergarten of America is in

many ways untrue to Froebel's spirit. For example, the doctrine of evolution and the recent advances in child study have not modified old practices. The kindergartners have been a sect of educational conservatives, given to the discussion of 'kindergarten principles' and suspicious of all others. The kindergarten "should cultivate children, not in pots, but in gardens." The author finds better conditions in Europe and thinks the tide to be turning in America.

The Educational Value of Dancing and Pantomime: Dancing is described as "the liberal, humanistic culture of the emotions by motions." It is not strange that in the history of play, war, religion, and love we find dancing and pantomime taking an important place, for all animal life in all its phases is predominantly expressive. What is strange is that we have been so ready to give over all this, classing it with the dance of the ball-room. The great wealth of folk dances should be saved to this generation. Every school should teach this new-old dancing, "even if it has to be open evenings for that purpose." Let them teach dancing of the face, arms and fingers, as well as of the lower limbs.

The Pedagogy of Music: Music is the speech of the "antique, half-buried racial soul," and through it "man may today dimly revive the most ancient elements and experiences in the history" of the race. As the child learns to speak before he reads, or studies grammar, so he should sing a wide range of songs before attention is given to notes and scales. Musical education "is to train the sentiments, to make children feel nature, religion, country, home, duty and the rest." There are many who do not have the natural ability to become musicians, but all should be trained to appreciate good music. To this end "a pianola in every high school and college" would be of great value.

The Religious Training of Children and the Sunday-School: In the teaching of religion the principle of psychogenesis should be applied. When religious instruction begins it should be largely through the stories of the Old Testament supplemented from non-Biblical sources. This will open up the mind for the life of Jesus, the New Testament and perhaps critical studies of other religions. The Sunday-school must see that while "religion represents the most vital part of the soul," "nothing so tends to lapse to formalism, routine and dogma."

Moral Education: "This is considered the most vital and the most difficult of all the many vast problems now before the American people. It is not for educators alone but for the nation to solve. It

is the problem in which all the deep questions touching the perpetuity of our race and people culminate, and one in which a great awakening seems by every sign to impend." If morals are to be taught, use should be made of "every source of personal, moral and religious enthusiasm" to which the child is capable of responding.

Children's Lies: Their Psychology and Pedagogy: Where the child's life is provided with that which will minister to a wide range of interests, where there are actual experiences in abundance, the tendency to lie has much less chance for harm.

The Pedagogy of Sex: This chapter deals with "the most difficult, delicate, and at the same time the most important part of moral education." The great body of sex thought in folk-lore is reviewed. Attention is given to the new epochs in sex psychology and pedagogy resulting from Freud's studies and the Mannheim conference. The topic is treated from the early sex life of the infant and child through to that of husband and wife, father and mother.

Industrial Education: The present vast waste of pubescents makes this the second question of importance. Few children have intellectual interests, while most of them are generally attracted by the actual things that the world is doing. Manual training as conceived and taught has failed to provide the point of contact between school and outside life; the natural interests in firearms, tops, kites, airships and the like have many unexplored possibilities. Present day vocational bureaus, business and trade schools and domestic training are discussed.

The Budding Girl: This period, between girlhood and young womanhood where nature begins to train and coach the female, is of peculiar interest and importance. It is the time when dress, freak appetites, secrets and affectations are prominent. School is accepted as a necessary evil, the boys are much more interesting.

Missionary Pedagogy: The missionary with his zeal to teach a message, with his desire to serve and willingness to undergo personal hardship and sacrifice, represents the ideal type of teacher. However, missionaries have usually failed to understand "what it means to fulfill rather than destroy." The spirit of the new missionary pedagogy may be represented by the question: "Is it needful for non-Christians to take the Christian name?"

Special Child-Welfare Agencies Outside the School; and Preventive and Constructive Movements: The many institutions and agencies which, though outside the school and church, are really for the education of children and adults, are considered in two groups: "the

first roughly comprising those whose object is more specifically relief and the palliation of evils already existing, and the second, those in which preventive and constructive work is the chief aim." These many agencies have been classified under the following titles or heads: defective children, delinquent children, dependent and needy children, hygienic agencies, moral and religious training, protective associations, recreation, child-welfare movements connected with schools, relief for sick children, and general welfare associations. These chapters are of special value to the social worker and the city teacher.

Sunday Observance: Sunday as a day of rest, recreation and meditation has a deep foundation in various phases of man's being. Suggestions are made regarding the Sunday use of libraries, museums, parks, school grounds and community playgrounds, also as to ideal evening meetings for interesting and enlightening laboring classes.

The German Teacher Teaches: There is a radical difference between the attitude and work of the German teacher who teaches, making "his own mind the source of most of the knowledge that the pupil gets," "and the American teacher who sets and hears lessons." Back of this there are differences in textbooks, illustrative apparatus and in educational conceptions.

A group of six chapters in pedagogy or methods in modern language, history, elementary mathematics, reading, drawing and geography form a most valuable section of the second volume. Every teacher who reads these chapters will be well repaid by a broadened view and a multitude of practical hints.

Pedagogy and the Press: The American press with its twenty-four thousands of publications in this country is an educational agency almost too vast to comprehend. Could not the teachers and editors get together? Why should not a large endowed educational institution publish a sample newspaper, free from sensationalism and suggested crime and vice? If the press would publicly discuss itself, many good results would follow.

Some Defects of Our Public Schools: School boards, the lack of professional training, teachers' salaries, coeducation, long vacations, length of daily school period, and kindred topics come in for a thorough criticism, and the chapter ends with the question, "Are we in earnest" to have our schools improved?

The American High School: This institution though new and lately established is thought to need remodeling in several respects. It should extend downward, perhaps to the sixth grade rather than upwards towards the college. The chasm between the two schools

should be bridged; for example, the reformed geography of the grades could continue with a new high-school dispensation of geology, chemistry, astronomy, and anthropology.

Civic Education: This final chapter is essentially one of hope and prophecy. The author sees much reason for encouragement in the present emphasis of service and civic righteousness. Here the school has an opportunity to be restored to its original social position in the state.

Educational Psychology. E. L. THORNDIKE. New York: Teachers College, 1910. Second edition, revised and enlarged. Pp. 248. $1.50 net.

In this revised edition the author presents what is really a new work on educational psychology. The general range of topics discussed is much the same as that of the work published in 1903. The chapter on The Influence of Special Training upon General Abilities has been omitted. Many of the chapter headings have been changed, and their order of presentation rearranged. Moreover the work has been thoroughly rewritten, in many places greatly amplified, and each section made to take account of the latest advance.

Individual difference is the theme of the book. "There is an incoherent mass of facts about the differences between one human being and another and the respective shares which sex, age, 'race' or remote ancestry, 'family' or immediate ancestry, and the circumstances of life have in the causation of these differences. It is the aim of this volume to put this last group of facts at the service of students." This, the author points out, is one of the four natural fields of educational psychology.

"Exact knowledge of the nature and amount of individual difference in intellect, character and behavior is valuable to educational theory and practice for two reasons. The first is the general need of knowledge of what human beings are in order to choose the best means of changing them for the better. . . . The second reason is that by a study of the causes of these differences education may hope to learn about means of making all men more wise, skillful and efficient. The causes of the differences between one man and another, as things now are, will lead to knowledge of the causes whereby all men may be made to differ from their former selves" (p. 3).

Following this statement of general aim there is a chapter on the measurement of individual differences wherein the author pleads

for 'units and scales' which shall admit of quantitative results. "A measurement of human nature to be useful for our purpose must identify the *amount* in question for any competent thinker, just as a useful description must identify the object in question." There are already too many studies which profess quantitative results while the conclusions rest altogether on subjective opinions.

The succeeding five chapters are devoted respectively to the influence of sex, race, family, maturity, and environment as causes of individual difference. The reviewer cannot do better by these full chapters than to use Professor Thorndike's words where he concludes: "Thinkers about the organized educational work in church, library and school need especially to remember three facts." (1) "For the more primitive and fundamental traits in human nature such as energy, capability, persistence, leadership, sympathy and nobility, the whole world affords the stimulus, a stimulus that is present well-nigh everywhere. If a man's original nature will not respond to the need of these qualities and the rewards always ready for them it is vain to expect much from the paltry exercises of the schoolroom." (2) "The channels in which human energy shall proceed, the specific intellectual and moral activities that shall profit by human capacities, are less determined by inborn traits. The schools should invest in profitable enterprises the capital nature provides. We can not create intellect, but we can prevent such a lamentable waste of it as was caused by scholasticism. We can not double the fund of human sympathy, but we can keep it clear of sentimental charity." (3) "Morality is more susceptible than intellect to environmental influence. Moral traits are more often matters of the direction of capacities and the creation of desires and aversions. Over them then education has greater sway, though school education, because of the peculiar narrowness of the life of the schoolroom, has so far done little for any save the semi-intellectual virtues. The one thing that educational theorists of today seem to place as the foremost duty of the schools—the development of powers and capacities—is the one thing that the schools or any other educational forces can do least. The one thing they can do best is to establish those particular connections with ideas which we call knowledge and those particular connections with acts which we call habits" (p. 141).

Some thirty pages are devoted to a presentation of individual differences in single traits. Here the author amply illustrates and emphasizes the point that where the scale of measurement is suffi-

ciently discriminative individuals are not found to fall into distinct classes or types, but that usually (not always) there is a distribution similar to the probability curve.

The three excellent chapters: The Relations Between the Amounts of Different Traits in the Same Individual, The Nature and Amount of Individual Differences in Combinations of Traits (Types of Intellect and Character), and Extreme Individual Differences (Exceptional Children), receive the same careful scientific treatment, the methods of exact science being applied throughout the book.

Professor Thorndike refers to more than sixty studies which have practical-theoretical bearings upon education. He usually calls them 'sample studies' and incorporates the kernel of them by way of illustration. This brief presentation of the important results obtained by other men, along with his criticism of method and conclusion, is not the least valuable point in the work.

The author's "hope" of presenting "a serviceable quantitative treatment of educational psychology" is assured by the present volume. The promised volume on man as a species—treating the general problems of instinct, habit, learning, practice, memory, fatigue and the like—will surely be warmly welcomed.

Principles of Education. F. E. Bolton. New York: Charles Scribner's Sons, 1910. Pp. xii+790. $3.00.

An abstract and general treatment of the large educational forces which play upon society forms the opening chapter. The aim of education is stated as "aiding the individual to come into full possession of all the desirable features of his heritage, to minimize the undesirable ones, and to initiate new tendencies" (p. 11). The breadth of this aim is accountable for the large range of topics presented.

In a general way the fore part of the book is devoted to biological considerations. Adaptation and specialization of functions, development and specialization of the nervous system, the theory of recapitulation, the culture epochs theory, fundamental to accessory, and inheritance are characteristic phases here treated.

Following this there is the psychological section comprising some seventeen chapters, which are devoted to a discussion of the educational bearings of individual differences, memory, imitation, imagination, feeling, volition and the like. The final chapter consists of a brief presentation of several subordinate topics such as would

come under the head of general discipline and educational values. No special section has been set apart for a consideration of the social phases of education, but the author has sought throughout to recognize the influence of society.

The work has been designed primarily as a text-book for college and normal school students. It will serve this purpose admirably. The method of treatment is pedagogical. The reader in beginning to study a topic is not confronted by a wall of technicalities. The author starts the discussion on the plane of the student by presenting illustrations and pointing out the general meaning and nature of the factors under consideration. Educational bearings and applications are discussed later, when the student has been prepared for them. The substantial appearance and size of the book are commendable. Students need to be impressed by the fact that educational psychology is a field far too large and rich to be compassed by a few weeks' cramming for examination. The size of this volume makes it possible to present much concrete material, which is the life blood of a subject, especially for the beginner. There is also the important point of introducing the student to the wide and varied fields of psychological and educational literature. These pages permit the incorporation of copious quotations from other standard works. The lack of a classified bibliography is in great part offset by the references which are given as footnotes.

We have here, gleaned from Professor Bolton's rich experience in the educational field, a work which is the best of its kind.

The Psychology of Education. J. WELTON. London: Macmillan and Co., Limited, 1911. Pp. xxi+507. $2.40.

The reader of this treatise is at once struck by the 'arm-chair' attitude and absence of references to current work and workers. Thus, in this whole large treatise the name of G. Stanley Hall is not mentioned. Yet the book commends itself to us very strongly as representing a much needed attitude—historical and constructive—in which relations may be seen without the encumbrance of masses of undigested material, and in terms of clear, logical generalizations which give us a true perspective. As a treatise on educational psychology, from an historical point of view, this book will stand in permanent value. Although there is nothing essentially new, the comprehensive and keen interpretation of the facts in hand makes it a manual which no educator or student of educational psychology today can afford to overlook.

An English reviewer has well said that "We have here no mere set of psychological facts followed by an attempt to apply them: we have education treated from the psychological standpoint"; and "Wherever keen insight, strong common sense, and long tried experience are necessary to lead to a true conclusion, there Professor Welton may be absolutely relied upon as a guide."

An Introductory Psychology, with some Educational Applications. M. S. READ. Boston: Ginn and Co., 1911. Pp. viii+309. $1.00.

Introductory Educational Psychology: A Book for Teachers in Training. S. B. SINCLAIR and F. TRACY. Toronto: The Macmillan Company, 1909. Pp. xii+180. $0.90.

Read's serviceable little book presents within a limited compass the "more important features of the structure and functioning of the mind," treating them in a straight-forward, elementary manner and with special reference to the reader's own experience.

There are several sections of the book, perhaps one fourth of it, devoted directly to educational applications. In fact, it has all been written from the educational point of view.

In contrast with the preceding volume the second is a manual. In no way exhaustive, it aims to be suggestive. Each topic treated is briefly defined and analyzed, calculated to draw out the student into thinking for himself on educational problems. The chapters usually end with two or three pages of maxims to be directly applied by the teacher. Unfortunately there are but very few references to the literature of the topics discussed, and even quotations are not assigned to their sources or authors. The book will be most likely to appeal to young teachers preparing for examinations in elementary psychology.

The Journal of Educational Psychology. Edited by W. C. BAGLEY, J. C. BELL, C. E. SEASHORE and G. M. WHIPPLE. Baltimore: Warwick and York. Published monthly except in July and August. $1.50 per year.

The establishment of this journal in 1910 under the direction of four prominent specialists who have the support of an almost unprecedented list of collaborators, marks a great advance for educational psychology. The value of having a special clearing house for the

exchange of information and for the correlation of all that concerns the relation of psychology to education can hardly be overestimated. The sixteen numbers already published have served to bring together a large amount of material which is of vital interest to this science. Only a few representative articles can be mentioned: E. L. Thorndike, 'The Contributions of Psychology to Education'; W. F. Dearborn, 'Experiments in Learning'; W. F. Book, 'The Rôle of the Teacher in the Most Expeditious and Economic Learning'; C. E. Seashore, 'The Class Experiment'; W. Van Dyke Bingham, 'The Use of Experiment in Teaching Educational Psychology'; G. D. Strayer, 'Measuring Results in Education'; E. B. Huey, 'The Binet Scale for Measuring Intelligence and Retardation'; G. M. Whipple, 'The Effect of Practice upon the Range of Visual Attention and of Visual Apprehension'; and Ernest Jones, 'Psycho-Analysis and Education.'

Zeitschrift für pädagogische Psychologie und experimentelle Pädagogik. Edited by E. MEUMANN and O. SCHEIBNER. Leipzig: Quelle and Meyer und Otto Nemnich. Published monthly. M. 10.

Die Zeitschrift für experimentelle Pädagogik and *Die Zeitschrift für pädagogische Psychologie* have been combined into this new journal. It will cover the major fields previously covered by the two. As these two journals have been pioneers in the effort to produce technical literature in the psychology of education, it is to be regretted that they have lost their individuality and are now to be devoted to more popular and general exposition.

BOOKS RECEIVED DURING SEPTEMBER

STARCH, D. *Experiments in Educational Psychology.* New York: Macmillan, 1911. Pp. vii + 183. 90 cents net.

Bulletin No. 3, Government Hospital for the Insane. Washington: Gov. Printing Office, 1911. Pp. 125.

ANDRÉ, E. L. *Ética española.* Madrid: Hernández, 1910. Pp. 304. 5 pes.

CUMONT, F. *Oriental Religions in Roman Paganism.* (With introductory essay by G. Showerman.) Chicago: Open Court Publ. Co., 1911. Pp. xxiv + 298. $2.

YERKES, R. M., and WATSON, J. B. *Methods of Studying Vision in Animals.* (Behavior Monographs, 1, No. 2.) New York: Holt, 1911. Pp. iv + 90.

STRONG, E. K. *The Relative Merit of Advertisements. A Psychological and Statistical Study.* (No. 17 of Archives of Psychology.) New York: The Science Press, 1911. Pp. iii + 81.

BREITWIESER, J. V. *Attention and Movement in Reaction Time.* (No. 18 of Archives of Psychology.) New York: The Science Press, 1911. Pp. 49.

WHITLEY, MARY T. *An Empirical Study of Certain Tests for Individual Differences.* (No. 19 of Archives of Psychology.) New York: The Science Press, 1911. Pp. iii + 146.

NOTES AND NEWS

THE University of Utah has this year established a separate department of psychology, and the new chair thus created has been accepted by Joseph Peterson, Ph.D. (Chicago, 1907). The department will, after this year, be housed in the $300,000 Central Building for which the state legislature has made an appropriation.

MR. CARL RAHN (Chicago) and Mr. A. N. Gilbertson (Minnesota) have been appointed instructors in psychology at the University of Minnesota. The material facilities of the department have been increased by the addition of two new rooms, to be used for laboratory and research purposes. Coöperation with the medical College is shown by the further development of the clinic, established two years ago

and now conducted by Professor Miner and Dr. Woodrow, with the assistance of two physicians. A new diagnosis blank has just been prepared.

THE present number of the BULLETIN, dealing with child and educational psychology, has been prepared under the editorial care of Professor C. E. Seashore.

The following items are taken from the press:

W. C. RUEDIGER has recently been advanced from assistant professor to professor of educational psychology in the Teachers College of the George Washington University.

PROFESSOR ERNST MEUMANN, of the University of Leipzig, has accepted a call to the Kolonialinstitut in Hamburg,

DR. STEVENSON SMITH, of Hampton-Sidney College, who has had charge of the psychological clinic at Teachers College, Columbia University, this summer, has accepted a position in the University of Washington, Seattle, Washington, to establish a psychological clinic in that institution. The position has been created in connection with the Gatzert Foundation for Child Welfare, and offers wide opportunities for usefulness. Aside from the work in the University, the Foundation provides for public lectures and an experimental school for teaching. Dr. Smith will have the coöperation of a large corps of physicians.

PROFESSOR E. B. HOLT, of Harvard University, has leave of absence for the present academic year.

DR. DAVID SPENCE HILL, of Peabody College for Teachers, Nashville, Tenn., has been appointed to the chair of psychology and education at the University of Tennessee.

MISS CLARA JEAN WEIDENSALL, Ph.D., University of Chicago, has been chosen to conduct psychological tests and investigations at the Bedford Hills Reformatory for Women, Bedford Hills, N. Y. Dr. Weidensall will carry on the work begun a year ago last summer by Professor Eleanor Harris Rowland, of Mt. Holyoke College.

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THE

PSYCHOLOGICAL BULLETIN

GENERAL REVIEWS AND SUMMARIES

VOLUNTARY PHENOMENA — EXPERIMENTAL

BY PROFESSOR R. S. WOODWORTH
Columbia University

Methods of Investigating Voluntary Action.—Three different means of bringing volition within the range of experimental observation have been introduced within the past year or two. Ach (1) interposes obstacles to the execution of an intended act, and so arouses an energetic will to perform it. Michotte and Prüm (2) require a choice to be made between alternative acts. Rowe (4), like Ach, selects for study an act that is difficult of execution, but the difficulty here arises from the unfamiliarity of the act, the method being properly described as genetic. Since a new field promises thus to be opened up for experimental study, a consideration of the methods is of first importance.

Each of these authors employs a combined introspective-objective method: after some brief act, the time of which is measured, the subject applies himself straightway to an introspective report. In the work of Ach and of Michotte and Prüm the introspection is what Ach calls 'systematic,' meaning complete, not directed towards any special point more than another. In spite of Wundt's scepticism regarding such introspection after the fact, the impression gained from the considerable mass of studies that have now followed this general method is that such observations have a fair degree of reliability. Certainly it is a mark of progress that generalizations vaguely precipitated from past experience should give way to induction from specific observations, even if the method of observation is subject to some difficulty.

These studies, like many similar ones on intellectual processes,

make use principally of the introspective data, relegating the objective measures to a distinctly secondary place. Statistical treatment of the objective measures is hindered by the excessive slowness with which they accumulate. Though the chronoscope may show that the process occupied but two seconds, the introspective review and record is likely to consume a quarter of an hour. The great benefit of combining subjective and objective observations, which would certainly seem to be the opportunity of *correlating* the two sets of data, will hardly be realized till the introspection becomes less 'systematic' and more sharply directed. Much more successful in this respect are the studies of Book[1] and Ruger.[2]

Ach's procedure is as follows. He first has his subject learn lists of nonsense syllables in trochaic rhythm, till the second syllable of each measure is strongly associated to the first. Then he presents the accented syllables, but, instead of requiring the reproduction of the associated syllable, instructs the subject to respond to each presented syllable by a syllable riming with it. The previously associated syllable tends to be spoken, and sometimes is not repressed in time to prevent a false reaction. After a few experiences with this procedure, and especially after a false reaction, the subject is led to a vigorous resolve to do what he intends and not to be led astray.

Selz (5), in a searching criticism, shows that certain unnecessary complications in Ach's procedure expose his conclusions to considerable doubt. He further raises the broad objection that Ach's method does not bring under observation the most characteristic of voluntary processes, namely the decision or choice, for the subject has decided on his general course of action beforehand, in accepting the instructions of the experimenter.

Precisely the choice between alternatives is the study of Michotte and Prüm, and their work is, as they themselves observe, complementary to that of Ach. Using the general technique of the reaction experiment, they showed the subject two numbers, with the instruction to choose between adding and subtracting them, or, in other cases, between multiplication and division. "Decide for serious motives, and then react," *i. e.*, press the key. The execution of the decision, the actual addition, etc., was not carried out, since the difficulty of this operation was apt to make it impossible to recollect the process of choice. But it was found that the process did not seem to the subject one of selection without action, for the reacting move-

[1] *The Psychology of Skill*, 1908.
[2] *Th Psychology of Efficiency* 1910.

ment of the finger appeared to him as the execution of his decision, like the giving of an order or the starting of a machine. Though it might be doubted whether 'serious motives' could intervene in so trivial a context, yet it was found that the subjects took their task with all seriousness, and experienced a definite feeling of activity in the choice and accompanying reaction.

The relation between this procedure and that of Ach is well pointed out in a supplementary note by Michotte (3). With Ach, the voluntary act preceded the stimulus ("I *will* respond to this coming syllable according to instructions"); with Michotte and Prüm the voluntary act fell after the stimulus. Partly on this account, perhaps, the reaction times average considerably longer in the latter study. With Ach, the volition is not immediately executed, but gives rise to a 'determining tendency,' operative after the stimulus; with Michotte and Prüm the choice issued instantly in a (symbolic) execution. With the latter, the choice was attended with difficulties and was voluntary; with the former, the choice was previously made, but difficulty attended the execution. In both cases the muscular tension attending the performance was resolved at the moment of execution; it was not terminated, in Ach's experiment, by the act of will, but only by the execution. The muscular tension (and feeling of effort), accordingly, seems correlated not specifically with volition but with readiness to act under difficulties.

Rowe worked more specifically with voluntary movement, seeking the 'cue' of such a movement. The movement studied should be unpractised, and with adult subjects this condition can be met by an unfamiliar *combination* of movements. Movements similar to those of typewriting were used; a keyboard was prepared by fixing ten small rubber bulbs to a board to fit the fingers, and to each bulb was assigned a letter by means of a visible key. Out of the ten letters was formed a short list of words, and this list was written repeatedly till the writing became automatic. Other experiments were made with handwriting, upside down and in other novel ways.

Results.—The wealth of observation and high level of discussion in all these papers make it impossible to do any sort of justice to them in brief compass. A few of Rowe's results, as bearing more directly on movement may be mentioned. In executing an unfamiliar movement, the 'cue' and control are perceptional, not ideational; attention was fixed almost exclusively on the 'copy,' the key and the bulbs — *i. e.*, on visual objects. Kinesthetic sensations were for the most part only marginally present, and imagery of the

movement to be made, in either 'resident' or 'remote' terms, was absent. With practice, the perceptual control dropped from the focus of consciousness, and its place was taken for a time, in some subjects, by memory images of copy, key, etc.; but with continued practice these images faded and attention was occupied with a grasp of the total situation, "an organized 'set' of consciousness in which the details no longer require attention in themselves but only as parts of a complex whole." The 'set' needs to be voluntarily maintained, and this is the sole remaining voluntary feature of the performance. Introspectively the 'set' appears as a blend of organic, tactile-kinesthetic and visual sensations.

The author concludes that the rôle of images, and likewise the rôle of kinesthetic sensations, in voluntary movement have been greatly overestimated.

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VOLITION AND MOTOR CONSCIOUSNESS—THEORY

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The appearance of an English translation of Bergson's *Essai sur les données immédiates de la conscience* (2) makes fitting an attempt to estimate the value of the seemingly new conceptions that he introduces into the treatment of the psychological problem of freedom. He distinguishes two kinds of multiplicity. The multiplicity of conscious states is not a discrete multiplicity, like that of number, but a qualitative multiplicity devoid of quantity. Thus the intensity of a simple psychic state is not quantity, but its qualitative sign. So also pure duration is the form which the succession of our conscious states assumes when our ego lets itself *live;* a qualitative multiplicity, with no likeness to number, and not measurable unless symbolically represented in space; an organic evolution which is yet not an increasing quantity; a duration whose heterogeneous moments permeate one another, forming an organic whole. But beset by the idea of space, we introduce it unwittingly into our feeling of pure

succession; we set our states of consciousness side by side in such a way as to perceive them simultaneously, no longer in one another, but alongside one another. The abstract homogeneous duration thus conceived is only the extensive symbol of true duration. The fallacies of both determinism and indeterminism arise from regarding intensity as a magnitude, not a quality; and from substituting for the concrete reality or dynamic progress, which consciousness perceives, the material symbol of this progress when it has lready reached its end, or, in other words, from applying the conceptual method to the act and its series of antecedents, *after* the act has been performed. Neglecting to eliminate duration from the outer world and space from the inner world, the one party has been led to deny freedom and the other to define it, and thereby, involuntarily, to deny it too. Freedom cannot be denied except on condition of identifying time with space; it cannot be defined except on condition of demanding that space should adequately represent time; it cannot be argued about in one sense or another except on condition of previously confusing succession and simultaneity. The inner states of the fundamental self are living things, constantly *becoming*, not amenable to measure, permeating one another, and their succession in duration has nothing in common with juxtaposition in homogeneous space. Hence the idea of necessary determination loses every shred of meaning, and the self grasped by consciousness is a free cause. This freedom can be formulated only as the relation of the concrete self to the act which it performs; and it occurs rarely, because the moments at which we grasp ourselves truly are rare. We cannot define it, just because we *are* free, and any attempt to analyze it unconsciously transforms the process into a thing and duration into extensity, puts the already done in place of the doing, stereotypes the activity and thus makes spontaneity settle down into inertia and freedom into necessity.

Bergson makes little of two concessions, better consideration of which, however, may rescue us from confusion, leave us still in possession of our deterministic science and still allow us to assign a definite value to Bergson's interpretations. If causality is taken as meaning, not necessary determination, but only regular succession, he says (p. 202), "we should willingly grant these philosophers that their principle is derived from experience, but it would no longer prove anything against our freedom." Again (p. 221), he asserts that time can be adequately represented by space [that is, in the abstract homogeneous manner] 'if you are dealing with time flown,'

but not 'if you speak of time flowing.' These are all the concessions that a valid deterministic science asks. Consciousness as actually lived, duration as directly perceived, freedom as really experienced, may be such as Bergson unfolds at length; and though he is not the first to have so insisted, he has performed a definite service in emphasizing this their living and flowing reality. But science, though it cannot live but must conceptually describe reality, substituting therefor 'an artificial reconstruction, a patchwork of dead fragments,' is necessary and inexpressibly useful. Facts permit and expedience requires that it be deterministic, if its determinism implies, as alone it justifiably may, only 'regular succession' and not necessary compulsion. And this holds for psychical as well as for physical phenomena, even though Bergson has justly argued that to completely foresee an action would be to be actually performing it, and that the same inner causes never recur in their actual wholeness and produce the same effects a second time on the stage of consciousness. In brief, for the concrete, self-permeating, heterogeneous, indivisible and unmeasurable, organically whole life of actual flowing experience, there is the practical but indefinable freedom of Bergson; for science, which we must use to conceptually describe this living experience, a determinism which does not deny freedom in any but the indeterministic sense, holds true.

Miss Wentscher (3) has given an excellent brief description and analysis of the phenomena of volition. Her point of view is that of deterministic science. Covering the entire field, her treatment is too complicated to summarize fully. Her different chapters deal with the apparent nature of will, as not only foreseeing, but also helping to determine; with reflex and impulsive activities; with the motivation of volition, both conscious and unconscious; with an analysis of the act of will, finding in it not a particular element of consciousness, but a complex of other conscious elements; with the development of will in children; with various consequences of the nature of will; with the question of voluntarily directed thoughts, and that of moral conflicts; with the problem of the energy of the will, and that of its freedom; with a critical examination of other theories of will than that which she advocates, including those of Ebbinghaus, Münsterberg, Spencer, Lipps, Maier, James, Meumann; with the results of experimental investigation; and with a concluding summary of the whole argument.

In the analysis of will, Miss Wentscher's particular merit consists in recognizing that it is not a special psychical element or power, but

a peculiar synthesis of ideation and feeling; that it involves not merely anticipation of the end or a mechanical result of the alternation and strife of motives, but also a determination of varying strength, and a complex of approval and encouragement of some features, disapproval and suppression of other features, based on the nature of the whole personality under guidance of its ethical norms existent rather as emotional worth-values than as logical judgment-factors. Her defence of determinism emphasizes rightly the fact that indeterminists merely insist on this factor of approval and encouragement of motives, but refuse to analyze the conditions on which it rests or to recognize that it has definite conditions; and shows, further, not only that it must have definite conditions in the total personality, but also that responsibility and the justification of punishment can have no other than a deterministic basis. Herein she agrees with Fullerton and others who have possessed clear insight into this phase of the problem.

Ach (1) has contributed an unquestionably valuable analysis of certain phases of voluntary activity. But he himself makes it clear that he is not here investigating the real problem of will, namely under what conditions of outer and inner compulsion (or, we must add, of regularity without compulsion) an effective will-impulse arises, but only of how, on the dynamic side, an accepted will-impulse passes into execution. There can be no question that he adds much to our knowledge of the definite details of this process.

It would be interesting to set forth in detail the factors described by Ach as entering into the execution of an act already willed, when the moment for its execution arrives; how obstacles to its execution are met, and how the success with which they are overcome serves as a measure of the strength and effectiveness of the determination; how the details differ in individuals of different temperaments, under the influence of different attitudes and feelings, and in cases which are extended, complete or abbreviated, immediate, delayed, or in course of execution, successes or failures; and to follow him again in his acute analyses of the later consequences of success or failure. But we can only mention that he treats these matters at length. As especially important from the theoretical point of view, it seems desirable to emphasize: first, that he holds that these details can never be given by the observer as matters of self-observation during the experience, but only as 'perseveriende Inhalte' after the experiment is concluded; and second, that much that is present can never be described more definitely than as a 'Valenz,' a 'Bewusstheit'

rather than a 'Bewusstseinslage,' an 'unanschaulich gegebenes Wissen.' In other words, the experimental-introspective inquiry always meets inevitably with later and thus possibly changed representatives of former actual experiences, and with unanalyzable and indescribable forms of experience which may turn out to be complex and describable, never for direct experimental examination, but only for theoretical evaluation. If this be true, the experimental inquiry, though unquestionably it is to be encouraged as adding indefinitely to our knowledge of detail, can never hope to solve completely the problem of the execution of an act of will. When it comes to the question as to the motivation of the will, the causeful or causeless origin of decisions in general, which Ach promises to deal with in a later investigation, it is very sure both that the experimental dealing with the problem will add much of value to knowledge of the details of the process, and will also miss inevitably the solution of the question as to its fundamental nature.

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VISUAL MOTOR FUNCTIONS

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It is suggestive that all the titles received by the reviewer are German. The coincidence need not be taken too seriously, however. American titles are plentiful enough outside the time limits of our review, and we happen to know of important work still incomplete. But this much is clear: psychological interest in the facts of eye movement, as contrasted with their 'introspective mythology,' as well as interest in the technique of objective methods for studying them, is far more general than it was a year or two ago.

In the papers which are chiefly medical, and which therefore may not be reported here,[1] Koch's work from the Kiel Laboratory is the only photographic study of the eye movements which is generally known and quoted. It is consequently variously unfortunate that, while he used the fairly satisfactory method of photographing the

[1] KOCH. *Arch. f. d. ges. Psychol.*, 1908, 13, 196–253.

corneal reflection, he neglected just enough of the technique to make his records misleading. The errors arise from the fact that vertical aberrations of fixation appear in his records as time.

A new comprehensive study of the angle velocity of the eye movements with present improved technique is badly needed.

Two studies come from the laboratory of von Kries. In the first Klara Grim (4) reattacked the problem of the threshold kinesthetic perception of eye movement. Inexcusably ignoring our present knowledge of visual fixation and the pursuit movements of the eyes, she holds with the Aubert tradition that the threshold in the perception of motion of a luminous point in a darkened room is the threshold of the kinesthetic perception of eye movement. The disturbing influence of autokinetic factors, and the secondary criteria given by long rectilinear excursions, are both cleverly minimized by confining the motion of the luminous fixation point to a circular path. This is undoubtedly a real advance in the technique. It is alleged to have the advantage of emphasizing the consciousness of eye movement through the continuous change in the relative tension of the eye muscles consequent to the circular pursuit. Independent of the questionable implications of kinesthetic threshold, the results viewed merely as a study of the threshold of the perception of motion are highly suggestive. She generalizes as follows: The smaller the radius of curvature of the path of the moving point the lower the threshold. Dark adaptation raises the threshold. Further ignoring the available data on the accuracy of fixation, and holding that the accuracy of eye movement is measured by the ability to count lines and dots close together, the author found the thresholds for horizontal movements between 9′ 32″ and 8′ 2″. It varies slightly for vertical and for oblique movements. The conclusion that (not only relative to its extent, but also absolutely) the accuracy of eye movement increases with the extent of movement might well have led to suspicion of the assumptions on which the evaluation of the experiments was based.

The second paper from Von Kries's laboratory by Marx (Leyden) and Trendelenburg (6) describes what is undoubtedly the most sensitive photographic technique for studying the fixation movements of the eye that has yet been published. A delicate mirror, attached to the cocainized cornea by an aluminum capsule, threw the image of a brilliantly illuminated slit on a suitable photographic recording device. A second mirror, fixed to the bridge of the nose, registered simultaneous head movements. In unenlarged records 1′ of eye

movement gave approximately 1 mm. displacement. The records show two kinds of intra-fixation movements. Minute tremors of approximately 1′, several times a second, are superimposed on longer irregular displacements of from 5′–8′, in 10″ of time. Involuntary head movements, within the limits of the head holder, also consist of tremors and longer displacements. But the long displacements of the head line are quite different from those of the eyes and do not coincide with them in time. The excellence of the technique, and the probability that the results will be quoted as the final authority on fixation movements, warrant a more extended critique. The author's assumption that eyes without the capsule would have less intra-fixation displacement is without any experimental basis. It is directly opposed by the recorded intra-fixation movements of other investigators. The theoretical exploitation of minimal values seems to be a questionable procedure. In view of the proven importance of extra-foveal factors in fixation, it seems absurd to assume that a black dot on a strip of white paper in a complex visual field constitutes a simple punctiform fixation object. Inspection of the published records shows pulse movements of the head in every head curve as well as in the one where it is specifically noted. The authors' assumption that this is capillary pulse is unwarranted. The angles of the curve seem altogether too sharp. The curves correspond better with the head movements consequent to the thrust of the big arteries as described in my experimental study of fixation. The misstatement that my technique shows an error of 10′ and is therefore not competent to show intra-fixation movements, is based on careless reading which would have been impossible if the authors had taken the pains to note the extent of the intra-fixation movements actually recorded by me.

Inouye (5), under the inspiration of Hering, measured the duration of positive and negative convergence movements by their apparent coincidence with the oscillations of a pendulum of variable length. The technique was exacting. Inouye was the only observer. Reliable results were found to be limited to minimal values. Movements of from 2°–12° took from 180σ–260σ. Negative convergence movements were found to be slower than positive.

An excellent device of Bárány (1) for measuring the torsion depends on the microscopic examination of the iris markings.

Bartels' regulation of the position of the eyes by the ear (2) is an exhaustive study with original experiments. The arrangement is by animal species.

Buys (3) describes a tambour, air transmission, recording device which is operated by the movements of the cornea, under the lid.

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RIGHT- AND LEFT-HANDEDNESS

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Lewandowsky (3) reports the following interesting case, which has an important bearing on the theory of the functional difference between the two cerebral hemispheres. A patient had a left-sided paralysis and other symptoms that pointed to a lesion in the right side of the brain, and at the autopsy a tumor was in fact found in the right centrum ovale. The patient also presented an aphasia and a right-sided apraxia. Such cases have been previously described in left-handed people, the normal functions of the right and left halves of the brain being evidently reversed. What was peculiar in the present instance, however, was the fact that the patient was throughout *right-handed*. A few previous cases have been reported in which a right-sided lesion in a right-handed person had given rise to aphasia, though the possibility has not received general credence, but this is the first case in which such a condition has caused both aphasia and apraxia; to such cases the name of 'crossed aphasia' or 'crossed apraxia' is applied. From this careful description it would seem that in future the possibility must be accepted of the speech and purposeful movements being localized in the right hemisphere even although the person is right-handed.

Klehmet (2) details the rare occurrence of an acute onset of left-handedness. The patient, a man of 21, was suffering from a catatonic hebephrenia engrafted on to a congenital imbecility of a light grade. Synchronously with the outbreak of the hebephrenic symptoms a total left-handedness appeared, for the first time in the patient's life; that he had previously been throughout right-handed was established beyond all question. The left-handedness extended to the carrying out of such difficult matters as bread-cutting and writing, the use of the left side was preferred throughout, and the patient frequently confounded the two sides of the body. The reviewer has no doubt that the occurrence has nothing to do with the usual left-handedness, but was purely psychogenic in origin and nature; he would further surmise that the case was one showing the dyschiric syndrome previously described by him—*i. e.*, it was a question of an hysterical symptom appearing in catatonia—in which the sudden and complete transference of the functions of one side of the body to the other is a well-established occurrence.

Bauer (1) publishes a very detailed study on the subject of the capacity to estimate weights with either hand. From his summary of conclusions the following may be abstracted. In general right-handed persons overestimate weights with the left hand, and left-handed persons those with the right. Overestimating of weights with the right hand may be an evidence of 'latent' left-handedness. The error in estimation, which even by normal people may be very considerable, is not due to any difference in the muscular power of the two sides, but to a difference in the 'Schwereempfindung' itself; it bears no relation to age, sex, or intelligence. The evidence collected speaks in favor of the 'Schwereempfindung' being a special sensation; it may be disturbed quite apart from any troubles in deep sensation (clinical sense of the term). Hypotonia leads to an error of overestimation, hypertonia to one of underestimation.

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REACTION TIMES

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Recent studies of reaction time, while they have brought forth no startling results, show well the great variety of the problems that may be attacked by this method. At the same time, they emphasize the fact that accurate results can not be obtained without careful attention to the technique of the method.

Dunlap and Wells (3) reopen the old question of the effect of intensity of stimulus on reaction time. "That differences of intensity, as such, within limits of what may be called satisfactory intensities, may cause differences of reaction time, is not established." They also attack the problem of why simple reaction times are commonly shorter for auditory stimuli than for visual. An answer to this problem is not reached; and it seems to the reviewer that the problem itself is not quite definite enough, in view of the fact that Wundt finds that the reaction times for sound and for light are about the same when minimal stimuli are used, and the further fact that a marked effect of both intensity and duration on reaction time has been found for light, and, generally, also for sound.

In this connection may be mentioned the results of Minneman (7) on the rapidity of perception of sound and light stimuli. Minneman finds that increase in intensity shortens the time of perception of both visual and auditory stimuli, but that it depends upon special circumstances whether sound stimuli or light stimuli are perceived more rapidly, so that one can not say in general that either mode of stimulus furnishes the smaller times. Dunlap and Wells did not measure the intensity of their stimuli, but state, with respect to their sound stimulus, that the sound was made decidedly loud, but not disagreeably so. The visual stimulus was the illumination of a white screen by the flash of a helium tube. They found in the case of simple sensory reactions that the visual reactions were longer than the auditory, but that when both the sound and the flash were presented simultaneously, with reaction to the flash—the attention being directed exclusively to the flash in the preparatory interval—the reactions were practically as quick as the simple sound reactions. That the comparatively short reaction time to the flash, when the flash occurred simultaneously with a loud sound, might indicate that the reaction was really to the sound rather than to the flash, is a natural explanation, suggested by the

authors, but not accepted by them. They refrain from offering an interpretation, until after further work.

In another article (4), Dunlap studies "Reactions to Rhythmic Stimuli with Attempt to Synchronize." By rhythmic stimuli are meant regularly recurring stimuli. The rate of succession of the stimuli, which were sometimes visual and sometimes auditory, varied from about ¼ to 2¼ secs. The reactions consisted in attempts to beat upon a practically noiseless key in time with the stimuli. As soon as the reactor began to receive stimulations, he prepared to react, beginning his reactions as soon as he caught the tempo. The recording, which was done graphically, began after about a dozen reactions. As regards the constant errors in the subject's attempts to synchronize his taps with the stimuli, they are found to be, as Cattell has remarked is so often the case with constant errors, extremely variable both in magnitude and direction. They show no very definite relation to the rate of the stimuli. As regards the mean variations, they are characterized as large, and found to be independent of the constant error, being as large, on the whole, when the constant error is small as when it is large. They are also as great, on the whole, at the faster rates as at the slower, thus making the short intervals relatively more irregular than the longer. The author urges the general similarity of this kind of synchronizing reaction and 'the characteristic process' in the complication experiment.

Henmon (6) reports some 'preliminary' results on "The Relation of the Time of a Judgment to Its Accuracy." The subjects had to judge concerning the relative length of two lines, 20 and 20.3 mm. long. The time elapsing from the presentation of the lines to the completion of the judgment (the latter indicated by a movement of the hand) was measured by the Hipp chronoscope. Immediately after each judgment, the subject assigned, in terms of a scale of four degrees, the amount of confidence felt in the accuracy of the judgment. A number of interesting conclusions are drawn, but even under the conditions of this particular research, the relation of the time of a judgment to its accuracy appears somewhat complex, so that further work, expressly outlined by the author, is needed before an unequivocal statement can be made.

From Külpe's laboratory comes a study by Westphal (11) of the structure of the subordination (Unterordnung) consciousness, that is, of the consciousness of persons busied with the simultaneous solution of 'Haupt- und Nebenaufgaben.' Thus, after a signal, the subject was shown an irregular polygon, and instructed to deter-

mine, as principal problem, the number of angles, and as subordinate problem, the longest side. Upon solving both problems, the subject released a key, and the time between exposure of the polygon and the release of the key was recorded by a chronoscope. The reaction times obtained in this manner are intended as a control upon the very extensive and detailed introspections of the subjects; but their value in this respect proves so slight, that one gets the impression that their measurement is itself merely a 'Nebenaufgabe,' carried for whatever may be got out of it. One interesting conclusion from the reaction times is, that, when the difficulty of the principal task is increased, more time is given its solution and there is no decrease in the certainty of the result, but if the task is a secondary one, and then greatly increased in difficulty, it usually suffers from neglect, so that there is no increase in reaction time, but, instead, more uncertainty and a greater number of incorrect answers. Moreover, the total time required for solution of both problems is usually shorter when the harder of the two is raised to the rank of principal problem than when it is carried merely as a side problem. The author urges the agreement of these conclusions with every-day experience. Reaction times for a purely synthetic method of solving the problems were found to be longer than for a purely analytical method. It was also found that it makes an immense difference whether the subject is told to react as quickly as possible or not, both in the time and the method the subject chooses for solution of the problems.

Wells, in a study on free association time (9), concludes that longer times, while apt to be the result of emotional coloring, may also be due to suppression, distraction, indecision, and the like. He finds the effect of practice (10) to be a decrease in the association time to a limit approximating 1.2 sec. At the beginning of practice, the subject may be any distance from this limit up to 3.0 secs. or more.

D'Allonnes (1) contributes an interesting criticism of the reaction time method as a clinical procedure in connection with the measurement of attention. He has devised for clinical purposes a form of reaction experiment not hitherto used, which, he believes, is free from objections that are to be made to the clinical use of simple reaction times, namely, too much delicate apparatus, the danger of automatic reactions, and needless precision in the time measurements.

Angell (2) and Dunlap (5) each have articles dealing exclusively with the technique of the reaction method. Angell presents data which confirm the conclusions of Breitwieser concerning the great decrease in simple reaction times that occurs with decrease in the resistance of the reaction key.

Salow (8) in an historical essay, traces in considerable detail the development of the reaction experiment, beginning with Bessel (1822). He refers to about one hundred and forty of the experimental studies made in this field since 1860.

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FATIGUE

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In spite of the obvious difficulties that beset such an inquiry, the question of the blood supply of the working muscle has not suffered neglect. Geissler and Zybell (1) report a series of experiments especially directed towards the question of the volume of the blood vessels under these conditions. The plethysmograph can scarcely be applied to the moving member, for, aside from the inherent difficulty of obtaining accurate readings under these circumstances, comes the consideration of whether the volume changes are the product of vascular ones, or of alterations in the distribution of the body fluids. The authors therefore employed the method of im-

mersing one arm of the sitting subject in a water plethysmograph with recording attachments, while the other arm executed a series of contractions upon a rubber bulb filled with water, continually emptying and refilling the same; the rate of the contractions varying from 30 per minute at the beginning to 20 at the end of an observation, which ordinarily occupied four minutes. Coincidently with the beginning of work is reported a fall in the plethysmograph curve, which may proceed throughout the four minutes, or be interrupted by a transient rise during the second minute. The pulse beats also become smaller. These changes are rapidly obliterated as soon as the work ceases, the normal balance being quite restored after some three minutes. Through continued experimentation the amount of fall in the curve is reduced to a small fraction of its original proportions. The finding of this negative change in the plethysmograph curve is, as the authors point out, contrary to the ordinary views upon the matter, which suppose a widening of the vessels to allow an increased blood supply to the working muscle. The authors consider it more probable, however, that the blood supply is proportional in the two arms, and that the resting arm gives a proper criterion for the working one. A psychological interpretation is offered, based upon observations of the relation of blood pressure to the intensity of voluntary effort. This, rather than the absolute quantity of work, determines the amount of blood pressure. The fall in blood pressure here observed is, then, to be taken as an expression of the *Unlustgefühl* that comes with fatigue; and its disappearance with practice goes hand in hand with the lessening of fatigue under these conditions.

In a later contribution to the same journal, these results are subjected to a searching criticism from the pen of E. Weber (6). The apparently simple and really very difficult method of the plethysmograph has resulted in a number of far from unimpeachable investigations that have tended to discredit the method among many. The use of a symmetrical member is strongly contraindicated, on account of the slight sympathetic movements that it induces. Relatively unrelated muscle groups should be employed; thus Weber has made use of an isolated movement of the foot and toes. Under these conditions is observed an increase of the volume of the arm, and not a decrease as above reported. Further objection is raised on the score of insufficient control of the breathing. It is not sufficient simply to endeavor to breathe regularly; without the actual pneumograph record such results can never be conclusive. In respect

to the origin of the phenomena, Weber's ideas are not so entirely different from those of Geissler and Zybell, for he also assigns to them a psychological interpretation, *i. e.*, that they are determined by the presence of motor imagery. In a hypnotized subject the possibility of actual movement was excluded, and vivid motor imagery of various limbs was suggested. There was observed an increase in the volume of the limb involved. Autosuggested imagery might have the same effect in the waking state; but passive movements, executed by another person, did not show the phenomena if the patient's attention were kept from them through hypnosis. Further, Weber speaks of simultaneous observations of the two arms, in which vivid motor imagery is suggested for one or the other arm of the hypnotized subject, motionless throughout. Ordinarily both arms increase in volume, the arm immediately involved more than the other; but if great emphasis is laid in the suggestion upon the quiescence of the other arm, it may undergo a slight decrease. Here Weber supposes the clue to the results of Geissler and Zybell. These workers, using one arm, would by all canons of plethysmographic method be compelled to be very careful not to move the other arm, which would be in the plethysmograph. As a result of this inhibition, vaso-constrictor impulses would reach this arm similar to those which Geissler and Zybell report. These inhibitory impulses are obviated by the use of asymmetrical portions of the body. Weber does, however, postulate that definite fatigue effects are shown in the plethysmograph curves, even to the extent of an objective indication of fatigue, so that unfatigued subjects alone are suitable for the conventional plethysmographic experiments.

The experiments of Imbert (2) are rather meagerly described, but the investigator appears to have made the observation that after continued (tetanic?) contraction of a group of muscles, brief relaxation brings about a very considerable degree of recovery. The phenomenon was demonstrated upon a number of subjects, including a group of soldiers, who showed some correspondence of endurance in this test with marching ability. He interprets the fatigue phenomena observed as due essentially to the reflex inhibitions from the pain involved. The observation regarding fatigue in tetanic contractions is not a novel one, and has been supposed to be due mainly to the exsanguination of the muscle as a result of its prolonged contraction; though some rôle, here as elsewhere, is doubtless played by the pain sensations.

Apropos of a six-day bicycle race in which drugging figured largely, especially, according to the author, among the American

participants, Loewy (3) prints a brief account of some experiments with reference to the combating of fatigue through pharmacological means. He is concerned especially with the consumption of oxygen and the production of carbon dioxide as associated with fatigue; seeking by chemical means to lessen the necessity for oxygen. The endeavor is to neutralize the acid products of metabolism, which earlier researches indicated to be practicable through the administration of spermin, a testicular preparation. The experiments reported are with two dogs, operating a sort of electric treadmill, the effect of work done under the influence of subcutaneous injections of spermin being compared with control experiments in which spermin was not administered. In the more extensive observations with the first dog, the relative consumption of oxygen is markedly diminished after the injection of spermin. The results with the second dog are less clear-cut though the author believes them confirmatory. He reviews also the work of Brown-Séquard which first suggested the dynamogenic effect of testicular substances, and which was confirmed by the observations of Zoth and of Pregl. They appear to have favored practice and recovery, and to have opposed fatigue. In the present experiments the spermin seems to have postponed the fatigue effects. The mechanism of its action may be twofold; it may chemically neutralize the acid products of fatigue metabolism, or it may greatly facilitate their removal along the normal channels.

In the mental multiplication of large numbers, Professor Thorndike (5) has developed an excellent method for the work-curve, in that so little portion of the experimental time is taken up with activities external to the test. It is limited in application, of course, by the fact that many individuals cannot perform the tasks at all. The present paper describes some of the properties of the work-curve obtained with this method, using three-place figures which the subject was allowed to see, and did not have to commit to memory. The results are scored in terms of time for a given number (4) of examples, with specific allowance for errors. Of the sixteen subjects, six worked from 3 to 8 hours continuously, the remainder 4 to 12 hours with pauses for meals. Then the work-curves thus obtained were compared with shorter performances on the following day. Three of the subjects seemed to do as well at the last of the work as after the rest period; these were also the three subjects whose total work periods had been shortest. The variability of this relationship is very great, the percentile figures ranging in the table from 61 to 297, though the different lengths of the work

periods make it inadvisable to fix a central tendency for the whole group. For a seven hours' work period, the ratio approximates 1.55. As the time of doing the work increases with fatigue, so do the number of errors; to cursory inspection it appears that rapid work in an individual has to some extent gone hand in hand with fewer errors, as often happens. For the cases available it is, however, quite certain that greater efficiency has been associated with resistance to fatigue; the subjects who do better fatigue less. If their efficiency be the result of practice, it would mean that in this function practice tends to reduce fatigability; the author does not apparently consider that it is so, but that the subjects may be taken as approximately equal in practice. Practice often overbalances fatigue for quite a distance through the work period. Allowing for the probable effects of practice, it appears that the cause of fatigue is a steady one, without sudden exacerbations. The question of the relation between the feelings of fatigue and objective fatigue loss is studied through introspective notes and receives its usual negative answer. The paper concludes with general tables giving the results by successive four examples for each subject.

While contributed only as a 'minor study,' the experiments of Norris, Twiss, and Washburn (4) deserve special mention from a methodological standpoint. They deal with the effects of fatigue on judgments of pleasantness and unpleasantness in color stimuli. "A state of fatigue," the account opens, "may naturally be expected to lessen the pleasantness of pleasant experiences and to increase the unpleasantness of unpleasant experiences." Very likely it is the feeling of fatigue that is here principally meant, though the actual condition also opens interesting possibilities. It was endeavored to observe how the judgment of color was affected by the subject's having to perform a long series of such judgments. The fatigue was presumably strictly confined to the mental work of the judgment, for any visual fatigue would obviously interfere much with the interpretation of the experiments. The ninety colors of the Bradley series were used as stimuli, and the subject was, upon presentation, asked to assign the affective value of the color to one of seven degrees in a pleasantness-unpleasantness scale. After judgments upon the ninety had been recorded, the first six colors were again presented, and it was endeavored to see whether the grades now assigned to these colors showed any constant deviation from the previous ones. There were thirty-five women subjects, college students with four exceptions. In seven cases the average position of the six colors

was higher for the second exposure than the first; for three it was the same, for twenty-three it was lower, with a range of .1 to 1.5 positions. There is thus indicated a slight downward tendency. Since the amounts of fatigue (*resp.* feelings thereof) are not uniform, and may even be negative, the small and variable differences do not reflect unfavorably upon the principle of the method. It is worthy of note that the changes in affective value observed are almost entirely confined to the shades and tints; the changes in the saturated colors are nearly balanced, though as frequent as in the others. No significant relation appeared between the tendency to change and the character of the judgment, though it might be shown in more elaborate experiments. The present ones are another demonstration of how readily many of the so-called *imponderabilia* of psychology lend themselves to a quantitative treatment, and the interesting character of the results to be anticipated from it. The brief study just described is replete with suggestions for further work.

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INSTINCT

BY PROFESSOR ROBERT M. YERKES

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During the year 1910 the most important general discussion of this subject occurred in a symposium at a joint meeting of the Aristotelian and the British Psychological Societies. The distinctive points of the views set forth by the five participants in the symposium may be presented.

Charles S. Myers (4, 209) contends that instinct and intelligence are inseparable; that each in its subjective aspect is found in every act. He thus briefly states his case: "Instead of defining instincts

as 'complex reactions which are perfect the very first time,' I have endeavored to show that they are all within very variable limits improvable by practice or by imitation, or are modified by changed conditions of environment. Further, in place of the usual definition of instincts as merely 'fixed innate activities,' I have maintained that the essential mental concomitants of instincts are the feeling of activity and a vague consciousness of the behavior to be achieved. I would urge that the existence of these central factors proclaims the presence of intelligence throughout instinct, and that, as the organism becomes endowed with an increasingly large number of mutually incompatible modes of reaction, the intelligent aspect apparently comes more and more to the fore while the instinctive aspect apparently recedes *pari passu* into the background" (p. 214).

The pith of C. Lloyd Morgan's contribution to the discussion (3), aside from his specific criticisms of Myers' view, is found in a note at the end of his paper. He states, in effect, that the control system of intelligence and the experience termed intelligent become differentiated from the mechanism and consciousness of instinct. "The performance of an instinctive act so stimulates the centers of intelligent control as to afford the primary data of experience. . . . It is therefore essential to distinguish between two orders of heredity: first, that which obtains within the automatic system, and which thus determines the nature of the hereditary responses; secondly, that which obtains within the system of intelligent control, and which thus determines the hereditary likes and dislikes. . . . I regard the cerebral hemispheres as the differentiated control system and conceive that they play no functional part in the automatism of instinctive behavior" (p. 229).

Taking exception to the main contention of Myers, H. Wildon Carr (1) presents a conception of instinct in its relation to intelligence which is essentially that of the philosopher Bergson. Instinct and intelligence, he states, designate interpretations, not observable facts. They differ not only in degree, but still more importantly in kind, for they are strikingly different modes of apprehending reality. "But besides the intellect, and implied in our knowledge of its limitations, is a power of intuition, that is, of apprehending reality not limited by the intellectual categories, and this reality is the living activity itself apprehended as a real duration" (p. 236).

G. F. Stout (5) enters the discussion with the following condensed summary and confession of faith. "Three main questions emerge in the papers of Mr. Myers, Mr. Carr, and Mr. Morgan. (1) Is

instinct a special way of knowing or is it a merely biological fact? Mr. Carr, following Bergson, holds it to be a peculiar mode of cognition. Mr. Morgan and Mr. Myers concur in regarding it as essentially a form of biological adaptation and as being, in this respect, on a level with reflex action. On this point, I agree with them as against Mr. Carr. (2) Is every intelligent action also instinctively determined? Mr. Myers says yes; Mr. Morgan says no. I agree with Mr. Morgan. (3) Is every instinctive action as such also determined by intelligence? Mr. Morgan says no; Mr. Myers says yes. I agree with Myers, and my position is here more radical and uncompromising than his" (p. 237). Stout regards instinct as a substitute for the capacity to learn. To him it seems relatively unimportant in man.

The conception of instinct defended by William McDougall (2) is markedly similar to the views presented by Myers and by Stout, and as markedly dissimilar to those offered by Morgan and Carr. "A typical example, then, of a purely instinctive action, implies the existence in the creature's innate constitution of, first, a specialized perceptual disposition; secondly, a specific conative tendency that is excited when this perceptual disposition is played upon by the appropriate sense-impression; and thirdly, some coördinated system of motor channels through which the conative tendency works towards its satisfaction. The three things belong together; each implies the other two; each can subserve the life of the organism or of the species only in conjunction with the other two; all three together constitute a functional unit which is transmitted as such from generation to generation; and to such a functional unit of the innate constitution only, and to no part of it alone, and to no other fact or feature of the organic world, can, I submit, the name instinct be properly applied" (pp. 264–265).

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SPECIAL REVIEWS

APPLIED PSYCHOLOGY

Influencing Men in Business. The Psychology of Argument and Suggestion. W. D. Scott. New York: Ronald Press, 1911. Pp. 168.

It does not often fall to the lot of a psychologist to be read as eagerly and as seriously by the producers and distributors of goods as by the members of his own profession. This has come to be the case however with William James and his *Principles of Psychology*. No small credit for this fact is due to Professor Scott and his clearly written popular presentations of many of James's analyses. The present book of 168 pp. elaborates his analysis of the chief types of decision, illustrating them by examples drawn from business life. Along with this is an exposition of the ideo-motor character of suggestion. Typical business situations are pointed out in which argument or suggestion would be most likely to be effective in influencing men. Suggestions for making arguments and suggestions effective follow, illustrated by advertisements.

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THE STEPPING REFLEX

Remarks on the Reflex Mechanism of the Step. C. S. Sherrington. Brain, 1910, 33, 1–25.

As might be expected of a presidential address to the Neurological Section of the Royal Society of Medicine, this paper is concerned with certain general conclusions which the author has reached with regard to the mechanism of locomotion rather than with detailed experiments. Although practically all of the experimental evidence from which the author draws his inferences has been published previously by him and by others, the analysis of locomotion and the interpretation of the experimental findings is new. Analysis of the act of locomotion reveals two opposed sorts of reflexes; the one postural, attitudinal or static, the other kinetic. *Static reflexes* may be evoked by appropriate stimuli in cats and dogs in which the brain stem has been

transected between the anterior and posterior colliculi. The muscles by which the reflex is executed are the extensors of the fore and hind limbs, the erectors of the spine and the elevators of the lower jaw. The efferent portions of the reflex arc are the motor neurones for the muscles named. The afferent portion of the arc is neither from the skin nor from the otic labyrinth, but it is from the proprioceptive system of the extensor muscles themselves. When the static reflexes are aroused after such an operation as that described, there results the condition called by Sherrington, decerebrate rigidity. This decerebrate rigidity is nothing more than the uninterrupted action of the static reflexes which are normally present in all pedal locomotion. Its function is to maintain the *usustatus* by opposing gravity. *The kinetic element* in locomotion depends upon the alternate action of extensor and flexor muscles. This *alternate contraction* of extensor and flexor muscles is achieved by *reciprocal innervation* and *reciprocal inhibition* of the two sets of antagonists, the seat of which is in the spinal cord.

Study of the decerebrate preparation shows that the kinetic element of the reflex is *rhythmic*. Faradization of an afferent nerve at a rate of 30 shocks per second produces a reflex response of 2 per second. This result shows that there is a *refractory period* during which arcs are inexcitable, a fact analogous to the refractory period of the heart. The normal stimulus to kinetic, as well as the normal stimulus to the static, reflexes is a proprioceptive stimulus resident in the muscle itself. Passive movement of a limb excites the reflex but not so effectively as an active contraction of the muscle. The relation between the extensor and flexor arcs is further studied in the phenomenon of *reversal* and the phenomenon of the *rebound*. The meaning of the reversal of a reflex is explained in the example which follows. The sartorius muscle is composed of two bands, a medial band and a lateral band. The medial band is inserted into the medial, posterior, surface of the tibia; the lateral band is inserted into the patella. The medial band therefore flexes the leg while the lateral band extends the leg. Passive extension of the leg causes reflex contraction of the flexor portion of the sartorius; passive flexion causes reflex contraction of the extensor portion. The phenomenon of the rebound is the exaltation of a reflex after its suppression by reciprocal inhibition. For example, the continued contraction of a flexor muscle inhibits the contraction of an extensor muscle. In decerebrate preparations, withdrawal of the stimulus producing flexion is followed by active extension. The rebound is due to a

post-inhibitory exaltation of activity in the previously inhibited extensor centers. Head observed a similar phenomenon in the diaphragm. Faradic excitation of the central end of the vagus which was just too weak to produce tetanus in the diaphragm was found to produce tetanic contractions of the diaphragm after the contraction of the diaphragm had been inhibited by inflation of the lungs. The locomotor reflexes which produce alternate postural and kinetic phases are self-regulative in a manner similar to the *Selbststeuerung* of the respiratory center, which was discovered by Hering and Breuer. This self-regulative quality is conferred upon the locomotor reflexes by the phenomena of reciprocal inhibition, the rhythmic response of the arcs by virtue of the refractory period, the alternate action of extension and flexion in virtue of the phenomenon of reversal and by the post-inhibitory exaltation of the reflexes.

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DISCUSSION

DR. YERKES' VIEW OF PSYCHICAL CAUSATION

Dr. Yerkes' *Introduction to Psychology*, which seems to be addressed to psychologists fully as much as to the novices in the subject, furnishes numerous grounds for profitable discussions of psychological theory. The most important of these battle-fields, it seems to me, is marked off by the topic of 'psychical causation.'

Dr. Yerkes' defense of the theory of psychical causality does not make clear just what he means by the theory. At least, I am puzzled by his statements and arguments. "There are two ways in which a psychological phenomenon may be explained," he says, "either in terms of other psychological phenomena, as one should naturally expect that it would be, or in terms of the physical conditions which accompany it. The former type of explanation is based upon psychical causation; the latter upon the correlation of psychical with physiological phenomena" (pp. 317–318) How shall a psychological phenomenon be explained in terms of other psychological phenomena? In the paragraph following that from which I have just quoted, Dr. Yerkes insists that the explanation is a matter of "knowledge of the circumstances or conditions under which a particular phenomenon appears," and that this knowledge "concerns relations of phenomena." Finally, if I have not misread, he limits the relations with which psychical causation is concerned to "the sequence of

mental events and the regularity thereof" (p. 319). He goes into details somewhat sparsely: "One image definitely and persistently leads to another; one idea to another; one sensation to another; a particular sensation is uniformly followed by a particular feeling, emotion, memory. Are not these instances of sequences of events similar to those which give the basis for causal explanation in physics? I think so."

If Dr. Yerkes had limited the application of the theory to associational sequences it would be intelligible, since all we know at present about these sequences is exhausted by the psychic facts. But Dr. Yerkes explicitly drags in the sequence of sensation and after-image. "How many hundred times have I observed that a certain visual sensation is followed by certain after-images?" he asks in this connection. It is this application of the doctrine of psychic causation which puts me completely at sea as to what it means, as well as to the nature of the distinction, in Dr. Yerkes' scheme, between the psychical and the physical.

Suppose we grant that the sequence of sensation and after-image is invariable, and is such that the law thereof may be stated passably well without reference to the structure and function of the eye. The sequence of sensations in the case of the flash and the roar of a cannon is just as invariable, and is just as capable of formulation without regard to other facts. In each case, the law of the sequence, if formulated without numerous reservations, is far from exact. Many flashes are not followed by roars, and in many cases a sensation of green is not followed by a pink after-image. Why, then, does Dr. Yerkes insist that the sequence of flash and roar is a physical relation, and that the sequence of sensation and after-image is a psychical relation?

If we can legitimately 'explain' the sequence of sensations and other contents of consciousness to which Dr. Yerkes refers in the statements I have quoted, without reference to anything other than the immediately experienced contents, why can we not 'explain' in the same way all of the millions of sequences in the world of phenomena — the sequence of color sensations when iron is raised in temperature; the sequence of tactual, muscular, visual, and auditory sensations when a gun is fired; etc.? Dr. Yerkes, be it noted, explicitly claims that all these phenomena are subject-matter for psychology.

I am not at all certain that Dr. Yerkes would not agree that these sequences are all capable of psychical explanation. If he were clearly committed upon this point the discussion would be much simplified.

But from the whole trend of his book I am forced to conclude that he would not and could not adopt this view. If it were adopted, there would be no point to the attempt to place psychical causation beside physical causation: psychology would be limited to the bald and inexact statement of observed sequences in the phenomenal world, and science would begin with the supplementation of the observed and observable facts by certain constructions which make the generalizations more exact and more readily handled. This is practically the position of Huxley in regard to the physical and non-physical ways of investigating the world.

The question of psychical causality rests ultimately on the question of the nature of physical causation. This, I think, no one will dispute. The main question of physical explanation is: Is it possible to explain causally without going beyond the observable facts? If it is not possible to give an explanation of the connection of the flash and the roar of the cannon which would be accepted as causal, without bringing in, explicitly or by implication, the logical symbols which are commonly referred to as *matter*, we may safely assume that there is a similar impossibility in the case of the connection of after-image and sensation. On the other hand, if it is possible to give a causal explanation in the first case without material reference, it is proper to consider the question whether that explanation is to be called 'physical' or 'psychical.'

The question of psychophysical causation, as distinguished from psychical causation, is also left in a somewhat confused state by Dr. Yerkes' treatment. There is never any serious dispute among psychologists as to what psychology includes; but the determination of what is psychical and what is not, is a treacherous task. It is regrettable that Dr. Yerkes does not set forth his principle of discrimination, especially since his application of the term 'psychological' is apparently more liberal than that of many of his fellow workers. Why, for example, does he refuse to assign a psychological character to the body? In what sense does he use the term 'body' and in what sense does he use the term 'mind'? These terms, unfortunately, have manifold meanings.

Certainly we can investigate the causal relations of sensations which we refer to external objects and sensations which we refer to the body, without stepping out of the parallelist's position. The flash of lightning and the start and exclamation of the frightened spectator; the perceived apple and the reaching forth of the hand to take it; these are objects of immediate experience, and may be correlated without raising the epistemological question as to the relation

between content of consciousness and changes in a material substratum. In these cases there is properly no question of psychophysical causation, but only the question of the possibility of an exclusively psychical causation as distinguished from the physical causation which we ordinarily invoke. Referring 'states of consciousness' to the concomitant 'bodily processes' may be a psychophysical procedure, or it may not: it depends on whether you refer to the phenomenal body or to the material body, and on how you define 'physical.'

Of course it would be unjust to charge Dr. Yerkes with responsibility for this psychophysical muddle, for he has simply introduced it by quotation from the standard authors who have muddled most thoroughly. My only complaint against Dr. Yerkes is that he did not make clear his own position in regard to the distinction between the physical and the psychological, and in regard to the distinction between the mind and the body. It is impossible to read his book without wondering what his criterion actually is.

In an article which preceded the *Introduction*,[1] and which seems to contain the same theory of causality which is embodied in the later volume, Dr. Yerkes has defined the physical as dealing with things from the "objective point of regard or attitude," and the psychical as dealing with things from the "subjective attitude," "as existent in the consciousness of the observer." This method of definition does no more than bring in two more ambiguous terms, for the distinction between 'subjective' and 'objective' is quite as vague as the distinction between 'mind' and 'body.'

In the attempt to make my meaning clear, I may point out that a way of distinguishing between the physical and the non-physical (which includes, and perhaps is exhausted by, the 'psychical'), might be based on the notion of causal explanation, or rather on a particular notion of causal explanation. I might claim that whenever we attempt to explain causally, or to deal with things in a way which implies a logical causal principle (matter, in the strict sense), we are dealing physically. When, on the other hand, we deal with things in a way which does not explain or imply a causal principle, we are dealing psychically (or at least non-physically). The acceptance of this principle, which I am not defending, would be a real step towards definiteness in the discrimination of the physical from the non-physical. Dr. Yerkes explicitly repudiates this principle. It is not clear that he adopts any other.

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[1] *Jour. of Philos., Psychol., etc.*, 1910, 7, 113–124.

A COMMUNICATION

MONOCHROMATIC LIGHT AND VISUAL ACUITY

Owing to the fact that the human eye is not achromatic, monochromatic light should produce a more distinct retinal image than light having an extended spectrum. The superior defining power of monochromatic light was shown in the following manner and published in detail in the *Electrical World*, Aug. 19, 1911.

The green line of the mercury vapor spectrum was isolated and used as a monochromatic source. This line was matched in hue by light having an extended spectrum obtained by filters used with the tungsten lamp. These two green colors could easily be matched for brightness without any of the difficulties which would obtain if they differed in hue.

A printed page of type of such size that at a distance of 1 meter it was just readable in monochromatic light, was arranged in a photometer so that two adjacent patches were illuminated respectively by the two green lights of different spectral character. For the same ease in reading it was found that the illumination having an extended spectrum must be increased 75 per cent. over the monochromatic illumination. This result was substantiated by other experiments. Later the Ives[1] acuity test object was used and by this more sensitive method it was found that for two observers the illumination having an extended spectrum must be five times greater than that of the monochromatic illumination for the same visual acuity. Another observer required an increase of only 33 per cent. It was shown that there was no movement of the pupil when alternately subjected to the two lights. Other experiments were made which bore on the subject of the use of the acuity method in the measurement of illumination. Photographs of the transmission of the filters used and a table of results are given in the original article.

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NOTES AND NEWS

THE meeting of the American Psychological Association will be held in Washington, December 27 to 29 inclusive, under the presidency of Professor Seashore. Joint sessions are being arranged with Section L of the American Association for the Advancement of Science and with the Southern Society for Philosophy and Psychology. This latter will be held at the Government Hospital for the Insane, at the invitation of Dr. Franz. It is probable that a sectional meeting devoted to papers on animal behavior will be held jointly with Section F. Arrangements have already been completed for a symposium upon the demarcation of the concepts, instinct and intelligence. The official headquarters of the Association will be at the Ebbitt House.

DR. F. LYMAN WELLS has severed his connection with Ward's Island and has returned to his former position at McLean Hospital.

THE following items are taken from the press:

AT Cornell University, Dr. Guy M. Whipple has been advanced to an assistant professorship of the higher grade, with the designation 'educational psychology' in place of 'the science and art of education.' Professor Whipple has assumed the editorial direction of the series of Educational Psychology Monographs, published in connection with the *Journal of Educational Psychology*.

DR. L. R. GEISSLER, of Cornell University, has been appointed associate psychologist in the physical laboratory of the National Electric Lamp Association, Cleveland, O.

AT Northwestern University, Robert Harvey Gault, Ph.D., has been advanced from an instructorship to an assistant professorship in psychology and has been appointed editor of the *American Journal of Criminology*.

ALFRED BINET, director of the Psychological Laboratory of the University of Paris and editor of *L'Année psychologique*, died in Paris, on October 19, at the age of fifty-four.

DR. JOHN HUGHLINGS JACKSON, the 'father of English Neurology,' died in London, on October 7, at the age of seventy-six.

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THE

PSYCHOLOGICAL BULLETIN

SOCIAL CONSCIOUSNESS AND ITS OBJECT

BY PROFESSOR E. S. AMES

The University of Chicago

It is the purpose of this paper to discuss social consciousness with reference to its function in the development of consciousness of the self and of particular objects, and especially in building up the consciousness of a Group Self or Ideal Personality. The first two problems have had considerable treatment in recent psychological literature, but the further implications of this general point of view remain to be worked out.

It is fairly well accepted that the individual comes gradually to self-consciousness through his interaction with other individuals. Every human being, if he is to live at all, is, from infancy, surrounded and cared for by persons. These persons fit into and help to constitute a social group. The child is nourished, sheltered, guided and disciplined by this human environment. All objects and influences are mediated by the persons near him. His very sensations are determined and modified by them. These persons are the moving objects in his field of vision and are therefore the first to catch his eye and to furnish vivid tactual and auditory sensations. As he becomes old enough to appreciate it even dimly, he finds himself talked to, and talked about, now made the center of attention and again ignored by the grown-ups. He exerts influence over these others by his cries, calls and antics. He discriminates between them and discovers that he has peculiar charms for certain ones. All such experiences contribute to the polarization of consciousness in the ego and alter, the self and others. This process continues to be elaborated through life, at least during any vital contact with other persons. The self is thus always changing in consciousness, and does not attain a final form or completeness. A permanent,

unmodified self would be the counterpart of a fixed and changeless social order, if indeed there could be any consciousness whatever in such an order.

Further light has been thrown upon this operation of social relations in developing the consciousness of self, by the investigations of the actual modes of communication between individuals.[1] The study of language has yielded fruit here almost in direct proportion to the emphasis put upon the dynamic, motor phase of speech. The approach to the more abstract word-symbols from the side of instinctive reactions and sign language has given unity and simplicity to the interpretation of the psychological process. Gesture is the term employed to designate any movement, sign, or vocalization which conveys meaning. In sign language, as in the signals of a train crew, or in the motions and facial expressions accompanying animated speech, the gestures are reduced or 'truncated' until they are often merely vestigial in character. Originally, the vocalizations were quite secondary to the movements and bodily attitudes, as is now the case with dogs and monkeys in their play or fighting; but spoken and written words are so suited to more elaborate and refined communication that they have become dominant in developed human experience. If, then, speech is regarded from the standpoint of the physiological adjustments involved, such as the action of the vocal organs, and the subtle, facile play of facial expression and bodily attitudes, it is apparent that this inclusive use of the term 'gesture' for all forms of direct communication is not forced or arbitrary.

Now, it has been shown that the consciousness of self arises in carrying on this interplay of gesture and response. The particular point in the process where such reflective consciousness appears is in the control of one's own gestures in reference to the gestures or responses of another, or of several other persons. One becomes aware that the other reacts according to one's own movements. One sees himself from the other's standpoint. This entering into the consciousness of other persons and making comparison with one's own, is essentially the social consciousness. In competitive games of skill and prowess an individual is thrown back upon self-analysis to determine how he may direct himself to outwit his opponent or to outdo his best efforts. He may secure the assistance of a trainer and coach. It is their business to help him to become aware of his weak points by viewing himself as they view him. They bring him

[1] G. H. Mead. 'Social Consciousness and the Consciousness of Meaning.' Psychological Bulletin, 1910, 7, 397.

to consciousness of himself by opposing him, by giving him practice in self-direction. He secures by this friendly criticism and testing a familiarity with his own sensations and movements, which enables him to improve and to realize a larger and more efficient self.

The use of oral, and especially of written, speech affords an incalculably great variety of social contacts and associations through which the personality of the modern individual is developed. Such a range of comparison, coöperation and conflict is altogether beyond the limits of sense perception, and is necessarily the work of the imagination. Here, the give and take of actual conversation is repeated among the people remembered and imagined. This dramatic rehearsal and anticipation of actual events in the mind is a genuine social experience and is quite as significant in its contribution to self-consciousness as the original upon which it is fashioned.

If we turn to the accounts given of our consciousness of physical objects, we find that here too the social consciousness is regarded as the determining condition.[1] The only way in which objects come to have significance for us is in reference to our conduct, and that conduct is social in its nature. It is obvious that the individual gets his introduction to objects in childhood, and practically through life, by means of the social medium. The uses of objects, their names, their values and their properties are designated for him. The child's frequent confusion of the labels and meanings of the adult world are amusing to his elders; but with maturity, his mistakes cease to be entertaining. He is required to conform, and if he should still persist in employing the fanciful and chance associations of childhood, he would be deemed unfit to share in the normal world of men and things. He cannot longer call ink-spots, 'buttons'; or confuse stacks of oats with 'dishes of ice cream.' He is bound to regard these and all other objects in the orthodox manner of the group to which he belongs or suffer real inconveniences and penalties. Books must not be apperceived as fuel, nor street lights as targets.

One of the greatest difficulties in realizing this social determination of individual perception is the failure to recognize sufficiently the dynamic, functional character of perception. Owing to our long established, facile habits of seeing, touching and manipulating things, we have scarcely any consciousness of these acts through which alone the objects in the world about us are 'given.' Since the process of knowing them is overlooked, the objects appear to stand

[1] Josiah Royce. 'Self-consciousness, Social Consciousness and Nature.' *Studies in Good and Evil*, Chapter VIII.

there stark and stiff in space. It seems the depths of metaphysical debauchery to assert that these physical realities have their being in the social consciousness. But when perception is stated in functional terms, the way is opened to an appreciation of its social aspect. Functionally the object is always conditioned by its uses. A sword is a weapon for hewing and piercing the enemy. Hung upon the wall of one's 'den,' it is a shining ornament. If it, and all objects of its class, had never been used except in this way, a sword would have no suggestion of blood and carnage. This principle applies to the inmost nature of steel as well as to its form. The physical properties of it are the formulæ of certain reactions obtained in the laboratory in the solution and statement of the problems of the physicist. 'Steel' is the name for definite forms of behavior, describable phases of experience. Its very hardness is a declaration of characteristic uses. These uses have been generalized, simplified and associated into definite sensations of touch and strain so that we are accustomed to feel of the metal and upon the ground of that feeling to pronounce the object hard. We do not take into account the long, complex and obscure process which has contributed to the facility with which we pass from the sensation to the judgment. The recovery of that process is difficult and, in its entirety, may be impossible. But that there is such a process and, in large part, *what* the process is, have been brilliantly and permanently recorded by modern psychology. Every object perceived *means* something to the perceiver. That meaning is of the essence of the object's reality. To be sure, the meaning may change but that is only to say that the object changes as well as the self perceiving it. All this is only to assert and insist upon the trite doctrine that all objects of our experience *are* objects of our experience, and that it is futile to discuss them, even as existing, outside of this experience. Few persons, however, are able or disposed to analyze and reconstruct in conscious reflection this subject-object relation. And of those professional psychologists who do accomplish it, many are not interested to follow out the implications of the facts. The result is that the naïve assumption of the self somehow related to the brain, with an object over against it, out there in space, continues to fortify the object in its mysterious isolation. Its inner essence is thus allowed a rôle of great importance, though in fact it is a sham and delusion. Its 'substance' is all conditioned—that is, relative—but the conditioning operation is constantly overlooked and an entity is posited as existing in its own right.

Now just as the object possesses and preserves its identity and reality only as an experienced object, so the self for which it is a presentation has its nature and function within a social order, as has been stated above. The case is not adequately expressed by pointing out that the perceiving self is one of many similar selves interacting upon each other. The mind of the individual is a social affair through and through. The instincts of man are social and their fulfillment involves the realization of a group life. All the processes of emotion and knowledge are phases of this social experience. The objects of perception are registrations of these group habits and activities. Their uses and names are fixed by the group's struggle for existence and the accompanying activities. In order to live and be in good standing with his people, an individual must live their life, see nature with their eyes, and keep the ancestral meanings for the things encountered. Departure from this way of viewing the world easily puts one out with his fellows, makes him appear uncanny and dangerous, if not criminal. The control and direction which social acts thus exert in sense-perception is comparable to the social aspect of the formation of language. The latter involves auditory and visual perceptions and motor adjustments. The words are as fixed as the things they designate. They have essentially the same definiteness and objectivity. To the primitive mind, words are real existences as much as sticks and stones, yet these words, to our reflective analysis, are social objects having no reality or function apart from the usage of a group. The physical objects of our environment are dependent to the core upon functional, social experience. The world of nature is our world, not as the possession of separate individuals but as the structure and operation of social consciousness in and through individuals. The self-consciousness of individuals is real and genuine but it has its reality within the social order and within the order of nature, standing related to both of these and they to it, in the manner just indicated.

I have dealt with the determination of finite self-consciousness and of physical things in the medium of social consciousness. I wish now to present some considerations concerning the development of group consciousness and the corresponding Object or Ideal Self, as these appear in reflective thought.

The process by which a sense of the group arises is not fundamentally different from that already described in the case of individual self-consciousness. Indeed the two develop together. The members of a group become conscious of the group as such through experiences

which put it into contrast with other groups, as in war; or the feeling for the group as a whole may arise when coöperation with other groups is planned, as in setting the bounds of the hunting grounds or in the common use of a water supply, or in the exchange of goods. In all such matters, the group is forced to take some account of itself, of its fighting strength, its base of supplies, its ceremonials, its industries and products as compared with those of neighboring peoples. The preparation of a tribe or nation for war is not unlike that of an individual contemplating a conflict with an enemy. There is the same envisaging of the situation, an attempt to anticipate the movements and strength of the opponent, a working up of emotion by recalling the injuries to be avenged and by tasting the sweets of victory in imagination. Perhaps it is in defeat or disaster that the group is most sharply stung into a realization of its needs. At such times the past is reviewed and vivified; old men are counselled; old ceremonials are reënacted; emotion rises to unusual pitch and overflows into channels long in disuse or cuts new paths into adventurous experiments. All such groping about, all such trial and error and partial success welds together the group, gives it a sense of solidarity and of corporate power. It is a significant fact that the group consciousness is most developed among those peoples which have preserved their identity against the greatest odds. The Jewish race is the stock illustration. Their race-consciousness has been strong enough not to give way under the strain of persecution, exile and ostracism, and has been hardened and annealed by these experiences.

Quite different results have appeared among those peoples inhabiting regions where the food supply was adequate and accessible and where neighboring tribes were sufficiently peaceful to require little, if any, military organization. Such conditions prevailed in the life of certain African tribes with the result that they developed scarcely any social coherence. Their customs were slight and insecure, their group-consciousness did not develop persistently and their tribal deities attained little definite character or influence.[1]

It is also of importance to note that the group-consciousness, like the consciousness of self in the individual, grows in the direction of stress and interest. Where war is the conspicuous and absorbing occupation of a people, it becomes conscious of itself as a military group and the attainment of that feeling about itself is registered in its war god. Where the cultivation of rice is a stable and well organized occupation, a rice deity appears. In a country where the

[1] Irving King. *The Development of Religion*, p. 95.

appearance of vegetation follows quickly and surprisingly upon the rainfall, it is natural that the alert and taut attention of the group should magnify the rain as god. Drouth and recurring showers, especially when the latter follow upon ceremonials designed to bring rain, serve to establish this religious object more securely. Here probably is the key to polytheism. Polytheism is the natural accompaniment of the numerous social habits or selves of the group. Since these habits have grown up under the pressure of varying interests, they have not been adequately coördinated. They persist within the life of the tribe or nation, alternating in importance and prominence according to the exigencies and fortunes of the group. The attainment of monotheism occurs where the life interests are simple enough or sufficiently unified to express themselves naturally through, and in reference to, one main activity or natural object. Some primitive peoples may possibly represent that status. But the significant monotheism of civilized man is only approached where the group life attains unity in its social organization, and is centered in the person of a monarch. In this type of society, the dominant function of the king has usually been that of leadership in war; but this has generally involved the necessity of caring for the food supply, transportation, public works and the codification of laws with reference to the various interests of the people. The tendency, under these circumstances, has been for the national consciousness to exalt the ruler as its highest objectification, and to deify him. The group then employs this representative, ideal Person, as a means of seeing itself in its moral and practical relations. He is another Self with whom genuine social relations are experienced quite in the same manner as with the individuals of the same or other groups. Thus the social consciousness is extended and enriched.

As the group thus comes to consciousness of itself through the organization and exercise of social control, the Group Spirit gains definiteness and idealization. The imagery and patterns through which the Group Spirit is represented are determined by those interests which are the foci of attention in the actual life of the people. Thus Yahwe, the embodiment of the Group Spirit of the ancient Hebrews, was represented as a Sheep in their pastoral period, as a Bull in a later stage, and finally as a King when the tribes were controlled by judges and kings.[1] It is important to realize, however, that in each instance the emotional attitude toward the object gave

[1] I have sketched this development in my book, *The Psychology of Religious Experience*, Chapter X.

it a significance far beyond what we may call its 'natural' character. The sense of group identity, the sources of material welfare, and the means of victory in war, were bound up with Yahwe under all these forms. Under the monarchy, he took on the new qualities of kingly wisdom and power, which were emphasized by the best interpreters of the national consciousness. As the human king became the agent and medium through which the social organism sought a fuller life, he became the type of that far loftier, wiser and mightier Being, loyalty to whom must determine the final prosperity and destiny of the nation.

This supreme Being or Object of the social consciousness is thus identified with the supreme social values of the group. In a pastoral stage of culture the highest social concerns center in the flocks and herds. The ritual and ceremonial of the tribe reflect this with the full color of life. The Hebrew religion to this day carries in its central ceremonial, the feast of the Passover, its original pattern, in which the supreme object is the Lamb. When the monarchical stage is reached the highest values of the social order are bound up with the wisdom and justice and power of the monarch. These qualities are therefore held to be the essential attributes of the Supreme Being. All the ritual of worship exalts and invokes these attributes, and the most fundamental demands upon the people are to respect and reverence them. In self-criticism, at moments of crisis, the reflective individuals of the group identify themselves with the point of view of this ideal Self, and thus bring their own conduct more clearly into consciousness.

In the very nature of the process here set forth the Supreme Being embodies the highest social values, and is in the highest degree personal.[1] These social values vary among different peoples, but, for a given tribe or race, it is not in the least difficult to determine what they are. It is also true that the degree and quality of personality differ in the deities of these different religions, but they possess all the personal elements which the group has achieved and learned to prize. It is not to be supposed that a group which has not attained to some definite consciousness of itself, and in the individuals of which there is no clear notion of self or personality, should be able to conceive their gods in personal terms. But it is just as inconceivable that the deities should be impersonal at a stage in which the

[1] I am at a loss to understand how this point should have escaped a recent reviewer of my book, cited above. See MARY WHITON CALKINS. 'Defective Logic in the Discussion of Religious Experience.' *J. of Phil., Psychol., etc.*, 1911, 8, 606.

social organism is discriminated, and in which individual consciousness has arisen. All of these phases of consciousness move forward together. It is only in a highly organized society, where there is much specialization of industry and refinement of the arts of life, that individuals of rich and well organized personalities may be found. Likewise it is only in such a social order, and for such individuals, that it is possible to find a highly developed conception of a personal God. It is probably true also that the development of a social consciousness, with the necessary self-conscious individuals experiencing it, involves the conception of a supreme Object, regarded as personal. If our conscious experience is through and through a social affair so that the very objects of physical nature are determined by it, it is inevitable that the Object of the group consciousness should be personal and social.[1] It is also inevitable that the idea of God, if it functions vitally at all, should grow in richness and vitality and not "tend to become faded and washed out as the development of society proceeds."

One is inclined to think that a serious difficulty in the minds of some of the critics of a functional, social psychology of religion is that they regard the explanation of the object as identical with explaining it away! But even Berkeley had a wholesome respect for objects while vehemently insisting that they are merely ideas! Surely no one thinks any the less of the value or importance of human experience since psychology has explained the soul. The explanation, in psychological terms, of the God-consciousness, does not destroy that consciousness. Such consciousness is the counterpart and implication of our social experience, in so far as that experience is organized and vital. Our minds are fashioned in a social medium and our intellectual operations are conversations from first to last. In scientific thinking, the process becomes highly abbreviated and schematic but never wholly escapes the interlocutory form. In moral reflections, where a course of action is sought, the process is often quite simply that of a discussion between the various selves involved, and not without the sense of an Ideal Self serving as an Umpire or Judge. In the experiences characteristic of developed religion this practical, personal attitude is dominant. One enters into immediate and vital relations with the Divine Personality, communes with Him, prays to Him and depends upon Him.

[1] This point is apparently overlooked by Professor Coe in his discussion of 'Religion from the Standpoint of Functional Psychology.' *American Journal of Theology*, April, 1911, pp. 304 f.

There is a radical difference between this attitude and that of the psychologist at the moment of making a psychological inquiry. But fortunately the psychologist is a man who is required to live in a practical way a large part of the time. If he did not know and practice the difference between being a psychologist and a practical man, he would not get through a single day. Suppose he started shopping as a psychologist and gave himself to his scientific reflections at every turn. If he kept to his task, he would probably become so interested in studying the process of perception while standing at the street corner waiting for a car, that he would not be able to get aboard when it arrived! His behavior, however, would not disprove the reality of the street car, nor the fact that other people actually traveled on it.

Our social experience is the basic phase of all our experience. Within it are gradually discriminated selves and things, the social group and the ideal Social Self. All of these are modified and developed with the growth of experience. An understanding of their nature and functions affords control for their further determination and use.

GENERAL REVIEWS AND SUMMARIES

RECENT TREATMENTS OF SOCIAL GROUPING

BY DR. ERNEST L. TALBERT

The University of Chicago

The factors which enter into the formation of groups are so diverse that the objective material must be sought for in many social sciences. A consideration of the various sorts of groups leads to such criteria as race, occupation, intellectual interest and national feeling, thus taking us far from a purely psychological account of the processes involved in the rise and maintenance of groups.

On the strictly theoretical phases, little that is fundamental has appeared recently. Some of the articles noted are semi-popular in mode of attack; they are included to indicate a drift of opinion valuable as content for the social psychologist, and to accentuate the necessity of further elaboration by means of hypotheses able to cover the complexity of the data.

From the usual crop of articles having to do with general classification of occupational strata and the moral to be drawn, the first selected is an attempt to distinguish the levels of contemporary English class structure (F. G. D'Aeth, 'Present Tendencies of Class Differentiation.' *Sociol. Rev.*, 1910, 3, 267–276). The lower division is composed of the 'loafer,' the unskilled laborer, the artisan, and the small shopkeeper and clerk. The second division, constituting a minority of the population, consists of the smaller business man, the administrator and professional group, and 'the rich.' The old demarcation, laborers, shopkeepers, and gentry (including the professions), has broken down; the ancient family basis of cleavage is succeeded by that of 'economic ability' and standard of life; the working class group has increased immensely, while the gentry has diminished. There is now a greater migration from group to group, due to widespread education, democratic feeling, and city environment. The writer makes an inconclusive characterization of the classes regarding income and mentality. He sees in the artisans "a solid, independent, and valuable class in society, possessing a very fair general intelligence; shrewd at times; a simple mind, not following

a connected argument; laborious procedure at business meetings" (pp. 270–271).

A severe indictment of 'the middle class' is written by R. G. Davis ('The Middle Classes and Social Progress.' *Westminster Review*, **174**, 379–382). Dilettante and motived by money, having neither the courtesies of the aristocracy nor the homely virtues of the laborer, this group lives an unreal, unproductive existence. In the working people are higher concepts of art, and their class feeling comes from the "relative failure of modern civilization which has not seriously touched the masses of the people" (p. 381).

The criticism is undiscriminating, and there is no convincing criterion of differentiation. Nevertheless, there is an enormous volume of sentiment abroad in the world which Mr. Davis voices. In France the dissatisfaction is evident in the attitude of Le Bon, although he is the defender of the bureaucracy. In the organ of the Le Play school, *La Réforme Sociale*, appears an article on 'La Formation sociale de la Jeuneusse des Classes aisées' (Paul de Rousiers, 1911, **62**, 369–384). The premise is that the élite is the middle class, that it is losing its grip, that it should decide to work and be capable of working. How to elicit 'a moral élite' the author endeavors to explain. The youth must be trained to bear responsibility early, must gain technical knowledge as well as moral and religious dispositions, and in loyalty to his group and to the state must emulate the solidarity of the labor union.

There is an increasing amount of attention given to the causes and implications of the 'class-consciousness' developed among occupational groups. The student of social psychology finds the labor movement of Germany illustrative because of its traditions, strong organization, press, and leaders. Group control, feeling, and leadership are sympathetically and critically discussed in *Das moderne Proletariat* (Eine sozialpsychologische Studie, by Dr. R. Broda and Dr. J. Deutch. Berlin: G. Reimer, 1910. Pp. v+226). In the preface the authors say that the book is the result of long association with those who feel most keenly the stress of the wage system. The treatment divides into three parts: (1) history and present organization of the labor movement; (2) description of the attitude of 'the proletariat' to religion, ethics, 'rationalism,' the family, nationalism, and 'Massengeist'; (3) personal confessions of working people, the authors endeavoring to use a kind of questionnaire method in order to get at the 'mind' of this group.

The discussion of class-consciousness follows well-known lines.

The writers recite the tale of monotony and weariness born of the machine industry. Medieval personality in product has vanished; the factory worker loses control of his time, his wages, and the conditions of labor. Alone he is a shrunken self; united with his class he is buoyant and strong. Not for himself but for a future coöperative society is he perfecting a group organization. He becomes more imaginative, intelligent, and hopeful because of this consciousness of unity, since he believes that his class is fundamental, destined to contribute a constructive point of view to all society. The discipline of poverty and the machine is the driving force, and the delineation of the effect of machinery upon the attitudes of the workers is similar to that given in Veblen's *Theory of the Leisure Class*.

This book, along with periodical literature, registers several changes in the concept of class-consciousness. There is realization that no healthful unity is possible by mere unanimity on all subjects. Varying temperaments are welcomed (p. 128). Rivalry and flexible organization are assured by the policy of having the three divisions of the mass movement, trades unions, social democratic party, and coöperatives, develop individual methods without interference and at the same time, in matters affecting the common welfare, to act as a body. A nice question is presented by the conflicting claims of nationalism and internationalism. Writers have speculated on the relative power of patriotism and devotion to the international occupational tie. Each has its associations, its symbols, and emotional appeal. The answer in this volume is a reconciliation. "Die Nation als Kulturgemeinschaft ist dem Proletarier nicht minder teuer, als dem Angehörigen einer anderen Klasse" (p. 137). But no modern nation is safe unless the international class-consciousness of its people is strengthened in order to neutralize the destructive tendencies in present society (pp. 135–138). Formerly, stress was put upon stimulating a mass spirit without appreciating the need of accurate knowledge of changing conditions and the counter reaction of opposing groups. Now it is seen that the consciousness of one group is in terms of the shifting attitudes of another. A significant feature of the book is the narraton of the evolution of 'ein naiver Massenglaube' to one deepened and guided by education and better organization (p. 113). The development of class and sentiment has a bearing on the earlier doctrine of an ultimate classless society and suggests the more tenable principle—that when a particular basis of class differentiation is found to be harmful it is the business of the bigger collective unit to replace a functionless grouping and preserve

conflict on a higher plane. In all the directions of change noticed the wholesomeness of group and sub-group alternate friction and harmony is demonstrated, and Professor Cooley's contention sustained, that a group-consciousness is an organization, not a consensus and motionless uniformity.

That class consciousness in the modern labor movement marks a distinct moral gain in the thesis of Vida Scudder (*Atlantic Monthly*, 1911, 107, 320–330). The broadening of sympathy and perspective is immensely superior to jingoism and clan exclusiveness. The working class tighten the lines to reach a humanitarian goal. According to Hans Müller, the atheistic bent of socialism is giving way to an admission that a bare dualism of class and mass cannot supply the sympathetic attitude which must underlie constructive movements. The leaders direct most attention to reform of economic structure ('Das religiöse Moment in der sozialistischen Bewegung.' *Sozialistische Monatshefte*, 1910, 3, 1665–1669). It has long been held by the exponents of economic determinism that the technique of production and exchange controls the religious, legal, and every other phase of organization. Ethnological researches do not altogether sustain the view. A denial that there is an exact correspondence between economic structure and political system is advanced by L. Michels. He argues that the evolution of particular groups is guided by factors at variance with the demands of economic prosperity ('Wirtschafts- und socialphilosophische Randbemerkungen.' *Arch. f. rechts. u. wirtsch. Philos.*, 1911, 4, 437–448).

Referring to the causes of group conflict within a state Max Huber points out that whenever the state loses touch with the demands of citizens, independent, hostile groups arise which may consolidate into an internationalism quite apart from national restrictions. The tendency to attribute legislation to personal and class aggrandizement is due to the critical bias of democratic nations. People place a higher value upon the community than upon law, and when the state seeks to repress a force which denies its autonomy, the groups concerned develop an 'international collective interest' ('Die soziologistischen Grundlagen des Völkerrechts.' *Arch. f. rechts. u. wirtsch. Philos.*, 4, 21–35). In the same vein John L. Gillin explains the conditions determining the rise of sects. He finds that fundamentally "sects are the result of forces stimulated to activity by a heterogeneity of the population of any social group. The lack of unity in the group results in the development of class-

consciousness" ('A Contribution to the Sociology of Sects.' *Amer. J. of Sociology*, 1910, 16, 236–252).[1]

A résumé of the present status of the problem of caste is given by D. Warnotte (*Bulletin Mensual*, Instituts Solvay, May, 1911, 238, 1–11). The caste system of India exhibits the principles of group decadence. For many years the Brahmans, by means of protective and isolative taboos, have kept apart. The segregation of groups in India has prevented the advance of national patriotism. It is well known that the English have drawn a number of their native officials from the Brahman class. Now the European education of the young Hindoo is leading to his denial of the sacredness of caste distinction, and he is using the ancestral devotion to a small group as a means of reaching a national consciousness. He recognizes that it is impracticable at present to include all classes in the exercise of political power, and he knows from European history that the élite spreads slowly among all classes. The objective point seems to be the glorifying of the 'ancient prestige' of India as contrasted with its 'present subjection,' and to utilize the high caste sentiment in order to oppose Hindoos to all 'strangers.'

The place of leaders in the control of groups is of much importance. Two representative tendencies of opinion are evident. As regards the function of leadership in the modern labor group, there is a rounded statement by Lüdwig Quessel ('Führer und Masse.' *Sozialistische Monatshefte*, 1910, 3, 1407–1412). The earlier view was that a leader should be a passive instrument yielding to the popular sentiment, that 'the people' were unqualifiedly superior political agents, that membership in an organization carries with it the capacity to pass judgment on all matters relating to the purposes of the whole, and that which concerns all should be decided by all. Trade unionists and social democrats in Germany tend to deny these assumptions. They hold that the feeling of solidarity must proceed from the corporate body, but in questions calling for knowledge of detail, tact, and judgment, nothing can take the place of the specialist. Primitive face-to-face oversight is no longer possible. The writer comments favorably on the English plan of restricting the use of the initiative

[1] There is an interesting reaction against making so much of the national and international consciousness. Several years ago Professor Royce advocated the worth of 'provincialism,' the formation of a 'local consciousness' for the sake of realizing the values of cosmopolitan interests. He intimated that our imaginative reach must have its base on small group relationships. *Race Questions and Other American Problems*, p. 107. Cf. Professor Hibben's recent defense of 'prejudice' and Cooley's discussion of the danger of superficiality, *Social Organization*, Chap. 10.

and the referendum, and of making the representative in parliament responsible for forwarding general policies, at the same time allowing scope to special ability.

Aside from recognition of the value of the leader's training, two advances are noteworthy. First, there is a denial that collective sentiment has automatic and predetermined channels for its overflow. Second, the leader's position becomes better defined. He is subjected to criticism from individuals in his group, a strong press, and hostile parties, and he is immune from freakish whims of popular feeling. Thus he is given leeway to act with initiative within a group whose traditions and purposes are fairly well defined.

Another view of the leader of the older political parties, following the school of Sighele and Tarde, is found in a work by A. Peterson (*Politik og Massemoral*, Copenhagen: G. E. Gad, 1911. Pp. 182). It is not yet available except through excerpts translated by W. Warnotte (*Bulletin Mensual*, May, 1911, 236, 1–10). The line of argument is not unique. It premises the vast rôle of the 'social milieu.' The special direction taken by society depends upon the temperament, the set of habit, and the 'material interest' of its members. There are leaders of temperament, of habit, and of material interest. In like manner parties are formed, dictated by one or more of these forces. It is the business of the leader to control his respective group by suggestion, appeal to primitive emotions, and general ideas meaning little but having volitional influence. However, the leader's purpose is conceived to be that which emanates from the 'mass' whose product he is. For the author the word *mass* replaces the French *foule*, and is defined as "a group of individuals who at a particular time are imbued with the same idea of aspiration and who have a consciousness of this community of thought, of wish, and of action."

A favorable proclivity is to explain the 'collective mind' under its various aspects of national, occupational, political, and religious bases of differentiation on the principle of line of attention, direction of interests, or special 'set' initiated by historical crises or accident. In discussing 'national psychology' Dr. R. Broda appears to depend upon an uncriticized conception of 'psychological type.' He discerns in the German labor movement a characteristic mysticism and emphasis upon education. In France and Germany the radicals are idealistic and full of 'Zukunfthoffnungen'; Australians show unemotional experimentalism. In America the spirit of initiative attending its political and economic evolution renders a segregated

class-consciousness difficult ('Zur psychologischen Differenzierung der internationalen Arbeiterbewegung.' *Dokum. d. Fortschritts*, 4, 262–269).

The concept of race is undergoing reconstruction; the criteria of the ethnological and anthropometrical schools are doubted. The assumption of fixed racial superiorities was assailed by Professor R. S. Woodworth in an address on 'Racial Differences in Mental Traits' (*Science*, 1910, 31, 171–186). He contends that not only the usual ethnological but also the as yet meagre psychological grounds of distinction between different races are inconclusive. The norms of 'type' and 'average' need to be checked up by that of distribution, since in two groups in which the average of abilities is equal the one with wider variations from genius to idiocy will rise to emergencies better (p. 173). Moreover, high rank in one direction may be accompanied by atrophy in another, and in a particular social circle, an approved sort of excellence is selected, ignoring equally good qualities. The psychology of peoples has not reached a satisfactory experimental basis, and about the only valid presumption is that instincts, emotion, and intellection are present in all groups. In tests by Rivers and others it is shown that in vision, color discrimination, hearing, reaction time, susceptibility to illusion, and power of attention, there are differences in individuals and some group superiorities, but when these results are corrected, there is slender justification for the theory of inherent racial qualities. Neither does the 'higher civilization,' *e. g.*, of the German as contrasted with the native Australian warrant us to assert his superiority, because other factors—accidental inventions, amount of leisure and quickness of communication—determine the preëminence of a people. Some experimental work showing the relative facility in learning when school children are alone and in classes is described by W. H. Burnham ('The Group as a Stimulus to Mental Activity.' *Science*, 1910, 31, 761–767).

Another discussion of the same drift as that shown in Professor Woodworth's criticism is essayed by Mariano-H. Cornejo ('Le Race.' *Rev. Inter. de Sociologie*, 19, 161–189). Professor E. A. Ross takes the sociological position that the 'Chinese mind' is not a changeless racial entity: it is explainable by reason of the impetus given by centuries of memory training and exaltation of habit. The deliberate, unimpressionable, precedent-loving disposition of the yellow man, his aversion to the restless Western 'scientific' attitude is not due to absence of capacity, but rather to the fact

that hitherto the 'psychological climate' has been unfavorable to innovating thought ('Sociological Observations in Inner China.' *Amer. J. of Sociology*, 1911, **16**, 721–733; 'The Race Mind of the Chinese.' *Independent*, Sept. 7, 1911, 526–528). Dr. R. Baron Budberg considers the patriarchal family and the consequent minimizing of individual responsibility to be the root of Chinese conservatism, since the Chinese state is the family written large ('Burg- u. Haftpflicht im chinesischen Volksleben.' *Globus*, 1910, **98**, 285–287).

Israel Cohen describes 'The Jewish Community' (*Soc. Rev.*, 1910, **3**, 216–226). He attributes the enduring homogeneity of the Jews to rigid religious prescriptions, an intense 'historical consciousness,' and the solidifying effect of persecution. Modern Jews are mostly urban, and in the midst of a dense population and a hostile environment lead a separated life. Until recently they have been debarred from civic responsibility, thus turning attention to commercial exploits and strengthening solicitude for their own group. However, liberal education and tolerance are tending to take the new generation away from the old Hebraism; the Jewish community, nevertheless, remains unique in its union of the historical and the religious consciousness.

A comprehensive statement of the 'Racial Element in Social Assimilation' is offered by Professor U. G. Weatherly (*Publications Amer. Sociological Society*, **5**, 57–76). The advance is from family or tribal cohesion to 'cultural unity.' The fiction of blood kinship shades into devotion to a common sovereign, and into the 'psychic sympathy' of a nation. But ethnic patriotism is still a factor in recent history and may break down national cultural unity. Nationality is due to sentiment growing out of a community of past experience and present interests. "It is not sufficient that people should merely have undergone similar experiences. They must have undergone them together" (p. 62). A common language is the great destroyer of racial separateness within a group. Immigrants, including the Jews, who come to the 'nascent social bodies' of such countries as the United States and Australia are quickly assimilated by the language, customs, and ideals of the new lands. The writer agrees with other students that "racial and geographical solidarity is already to a limited extent giving way to interracial and international class solidarity."

A suggestive paper which touches upon much of the theory of social grouping is contributed by Professor G. R. Vincent ('The Rivalry of Social Groups.' *Publications Amer. Sociological Society*,

5, 241–256). The group is regarded as a definite and workable concept, for it is a tangible subdivision of the vaguer 'society' and the 'nidus of personality.' The idea of a group presumes (1) a common interest, (2) a realization of the group as such by each member, (3) that the constituents of the group are aware of the common interest and know that the image of their group is shared by the others. In the meaning of group the author includes the boy's gang, the labor union, the church, the coterie, the village, and the nation.

Competition, conflict, and rivalry are the chief agencies which force human beings into groups. Each group, by suggestion, imagery, and other means of control forms 'permanent types of reaction'—the mores. Collective pride, type heroes, the elaboration of a 'protective philosophy,' ridicule, and intolerance are ways of meeting the conflict situation. Fashion is interpreted as a form of group rivalry. The 'mob mind' may have a functional value in solving a problem quickly; on the ideational level 'free speech' induces a prompt cohesion of forces against a competing group. "A national group is to be thought of as an inclusive unity with a fundamental character upon the basis of which a multitude of groups compete with and rival each other. It is the task of the nation to control and utilize this group struggle, to keep it on as high a plane as possible, to turn it to the common account. . . . It is in conflict or competition with other nations that a country becomes a vivid unity to the members of constituent groups. It is rivalry which brings out the sense of team work, the social consciousness" (pp. 255–256).

So much territory is traversed in Professor Vincent's sketch that the promised longer treatment is to be desired. It is evident that on the whole the literature reviewed is either entirely descriptive and classificatory or that the psychological machinery is not adequate to interpret the social facts. This is, of course, characteristic of the present state of social psychology, with a few happy exceptions. The treatments are inclined to be too psychological in the solipsistic sense or too sociological in the structural sense. It is assumed, following tradition, that the self is entirely inner, and that the problem is to get consciousness into that of another self. Another tendency is to suppose that a sufficient method is to stratify the phenomena of society into psychic planes and currents, and to give the individual a part to play by a mechanism of suggestion and imitation. A statement of the 'converse of attitudes' involved in group relationships and the motor process by which the development of feeling, symbol and thought as a social dialectic is really explained and not obscured by a deluge of words, is imperative.

The failure to give a satisfactory picture of a complex situation by the use of a halting psychology is striking when an attempt to apply a professedly social psychology to a social problem is made. No one has accused Gustav Le Bon of being unbiased, so that the employment of his latest venture for illustrative purposes will present the case somewhat exaggerated (*La Psychologie politique et la Défense sociale*). Like M. Lévy-Bruhl, who distinguishes primitive man's logic of the sentiments from the modern logic of the intellect (*Les Fonctions mentales dans les Sociétés inférieures*), he makes a cleft between the mob-like *Mentalité ouvrière* ruled by passion and the ideational scientific mind of the élite. According to his doctrine, individual psychology makes no contribution to the understanding of social facts, since *l'âme collective* differs in toto from *l'âme individuelle* (p. 126), Futher, sentiment and intelligence are disparate, ruled by different laws (p. 141). The logic of ideas cannot interpret the feelings. Social consciousness is fostered in the crowd; the élite increase in self-consciousness and lose in consciousness of solidarity. In the concluding pages, the author, apparently realizing that since ideas are lodged in one section of the individual and society and sentiments in another, there is no encouraging prospect for effective communication, resorts to another faculty, that of will. Just because the motor character both of idea and feeling is disregarded, the final deus ex machina must be a separate entity, *une volunté forte* (p. 375).

That there is a serious political situation in France is undoubted. It is doubtful whether on the basis of M. Le Bon's Cartesian philosophy it can be met. If we assume that the nearer we get to perception and feeling the lower down in worth we are; if we assume that the gregarious instinct, the unanimity of image and action in the mob, the losing of all the conditions of individuality, is a true characterization of what social consciousness means; if thinking is exclusively an isolating process and not an intercourse, it is no wonder that with Le Bon we are led to denounce, to despair, to demand scientific prevision of fatalities, and to appeal to pure force. The difficulty is that there is no motor technique to warrant us in advancing from sentiment to idea, and from the individual to the collective standpoint. Le Bon cannot admit that self-consciousness arises in a situation involving an analysis and interpretation of the attitudes of other selves, that this situation is first experienced as an emotional one, and that by alternate rivalry and adjustment the individual expands in feeling and cognition. It is legitimate, there-

fore, in opposition to Le Bon, to interpret the conflict between the crowd and the élite as evidence of expanding sentiment and idea in both groups, each party growing in knowledge of the situation and defining the issues. Politics for Le Bon is a process of imposing superior power and control devices upon a non-contributive group. Its reaction is of no consequence, since what it possesses is identified with the primitive pre-ideational consciousness. Perhaps it is itself proof of provincialism and social grouping if one turns to Professor Cooley's balanced treatment of social classes and their mutual contributions in Parts 3 and 4 of his *Social Organization.*

SPECIAL REVIEWS

SOCIAL PSYCHOLOGY

The Influence of Newspaper Presentations Upon the Growth of Crime and Other Anti-Social Activity. FRANCES FENTON. The University of Chicago Press, 1911. Pp. 96.

To get at the effect of the appeals made by the newspaper is the purpose of this thesis. The point of view is objective and functional. As a study in applied sociology, it aims to suggest ways by which "the control of stimuli to activity for the purpose of diminishing crime" may be obtained. Premising that 'orthodox psychology' has centered interest around conscious processes, to the neglect of unconscious presentations, Dr. Fenton criticizes previous treatments of the problem, formulates a definition of suggestion, and inquires into the conditions in the individual and in 'objective social conditions as stimuli' which facilitate the going over of presentation into act.

Two lines of analysis are used: (1) direct evidence of newspaper suggestion revealed in the confessions of the criminal, cases cited by the newspapers themselves showing stimuli to crime, statements of persons who deal with social offenders, and other evidence; (2) determination of the amount of space devoted by newspapers to accounts of anti-social activity. The cases cited strengthen the growing conviction that some newspapers have specialized on the spectacular and dramatic, have tended to break down the inhibitions of the weak, and to lower public standards. It is also demonstrated that the relative amount of space devoted to anti-social matter—from 5.91 to 20.02 per cent.—is not offset by constructive editorial discussion (pp. 48, 91).

The conclusion treats of the pecuniary motive in relation to the social function of the newspaper, and gives helpful recommendations for improvement. The treatment throughout is clear and vigorous, and shows the possibility of a scientific method of obtaining data which the social psychologist may confidently use when he essays to follow Professor Cooley's procedure, that of 'sympathetic introspection.' From the specifically psychological standpoint, the writer had made no contribution, and one feels that the analyses and quo-

tations from many authors could have been replaced with a positive statement of the concepts of suggestion, habit, 'control,' and attention without detriment to the argument. The orthodox psychologist probably realizes that the whole cycle of the act is more inclusive than our distinct awareness of it, and he knows that he has been most successful in introspecting and measuring the cognitive processes. And he may legitimately claim that without his analysis of the way our conscious life goes on, such 'objective' treatment as that of Dr. Fenton's would have no guiding point of view. He is indebted to the neurologist because of the light shed by the neurone on the conditions under which our conscious and unconscious activities continue. The psychologist is similarly grateful for this admirable interpretation of the social conditions giving rise to below-grade reactions to stimuli.

ERNEST L. TALBERT

THE UNIVERSITY OF CHICAGO

Sociology and Modern Social Problems. CHARLES A. ELLWOOD. New York: American Book Co., 1910. Pp. 331.

In this book five chapters are devoted to the problem of the family; other chapters discuss such matters as immigration, the city, poverty, crime, and education. The treatment is objective, concrete, and elementary, making the work valuable for the college student and for those whose specialties forbid an intensive cultivation of the field of social issues. Emphasis is placed upon the biological, statistical, and economic phases. In a sane fashion the necessity of understanding the ethnic and biological background of the negro and the immigrant is enforced. The author expressly confines himself to non-psychological sides of the questions outlined, although he does not avoid them altogether.

Like other enthusiasts for the new sociology, Professor Ellwood believes that it supplies a synthesis and general point of view for the special sciences. Its problem is that of the organization and progress of society. It handles in a general way what the older disciplines work out in an abstracted manner. It cannot ally itself with any narrow program of reform too closely, else it sacrifices its character as a science; nevertheless he urges that its ultimate aim is practical, although as yet it has not developed a satisfactory technique for the attack of complex problems.

There are several consequences of this combined non-psychological and synthetic mode of treatment which may be mentioned. The supposition is that sociology first develops its norms and then hands

them over to the moralist to be criticized and applied. There is danger of a difficulty here, even if we admit that ethics is, in any peculiar sense, a normative science. I cannot see that the norms of the sociologist have a distinguished place and validity for ethics, since it, as Professor Ellwood says, is becoming more and more social in its outlook. What the moralist takes over is material and interpretation from any source, not more or less fixed norms from a general science called sociology. The elaboration of the conditions and implications of the ethical situation is the province of ethics, and the facts and the interpreting norms of all phases of experience are, from the ethical standpoint, subjected to revaluation. There is a species of books on ethics, of course, which attempts a structural and synthetic account, forgetting the specific experience originating the whole problem. Such was the case with the followers of Bentham, but they finally issued in a standpoint reconciling motive and consequence. In other words, to eliminate the psychological in a treatment of social problems is to cripple the ethical, and to confuse the political background of conduct with the issues of a specific ethical difficulty.

A second illustration of the inadequacy of a non-psychological statement is found in the writer's exposition of the family. There is a fine appreciation of the fundamental character of this institution, and the sociological causes of its present instability are well canvassed. I realize that it is pedagogically important to uphold the sanctity of marriage and to point to statistics for confirmation. It is also important to discuss the matter from the angle of the development of attitudes in this small group. The family is the field for developing 'disposition,' ideas, and sentiment, and by taking successively the standpoint of father, mother, and child toward one another and toward encompassing groups in a discussion of its function, there is more possibility of harmonizing the subjective and objective factors than by relying overmuch on figures and biological data, however valuable they may be. Professor Ellwood does not ignore the educational and moral aspects of the family. Perhaps the point I wish to make may be illustrated by referring to the mode of treatment commenced in Chapters 10–13 of Helen Bosanquet's book on the *Family*, and by the concrete fact that some divorces in this country may be justified if one considers the effect of discordant family life upon the child's future social attitudes when from the objective sociological view there may be condemnation.

A final comment relates to the possible effect upon the student

and upon mature people of overdoing the sociological doctrine of 'interrelation of factors' and complexity of social problems. It may be that the intended purpose of inducing a broad comprehension and a hopeful working out of difficulties is frustrated. We fail to see where to take hold, and are not sure that taking hold will do any good. One comes away from reading the 'objective' scientific school of French sociologists with the impression that everything must be left to the savants who operate the social pressure. Our recourse is to our own psychological technique in lieu of the future technique of the sociologist.

All this says nothing against the excellence of Professor Ellwood's book within its aim and self-imposed limits.

ERNEST L. TALBERT

THE UNIVERSITY OF CHICAGO

The Individual and Society. J. MARK BALDWIN. Boston: Richard G. Badger, 1911. Pp. 210.

A brief résumé of Professor Baldwin's doctrines, especially that of the relation between the generalizing and the particularizing aspects of the life of society, together with some applications, is the content of this volume. Since there is no essential change in point of view, an extended notice is unnecessary. However, there is a tendency to insist upon the fact that the individual is no mere 'repeating machine,' and the central problem is: If the individual 'absorbs obediently and imitatively the matter of the social life and habit of his group' (p. 146), in what sense can he be said to 'invent'?

The answer is: (1) There is phylogenetic continuity; (2) there is an emerging of instincts and feelings parallelling the racial biological sequence; (3) these instincts in the individual are both of the friendly and of the hostile sort, thus guaranteeing the alternate separatist and coöperative tendencies by which growth in individual and society takes place; (4) in the child and in the adult there is an inherent tendency for response to reinstate the original stimulus; this is true all the way from perceptual up to deliberative processes; (5) the individual's departure from his copy is a matter of spontaneous improvement upon previous data by means of anticipative imagination; (6) a constant dialectic goes on; 'invention' is as natural as imitation (p. 149); 'idealization' produces variations which are selected or rejected by the group. Society proper appears when the biological and instinctive forms of connection are succeeded by the

conscious ideational. Progress consists in the reciprocal working of the conventionalizing tradition and the individualizing invention.

As a general schema, not much exception can be taken to the description, and the many ways of stating the problem make objections difficult, since another page will be cited. Notwithstanding the apparent just and balanced treatment, some readers of Professor Baldwin's earlier books will still be troubled by the crucial question—how the individual and his inventions can be explained on the basis of the mechanism offered. That the original deed and thought are not a bolt from the blue, that the reformer works within a group-consciousness, may be conceded, but just how, when, and why are not evident. A restatement of previous criticisms of the blanket use of the term imitation is uncalled for. If the mechanism which explains the child's response to the gestures of the nurse, however extended, is adequate to explain the symbolism of conception, then thinking seems to be nothing more than an assimilation of one's already determined self to an 'interacting' self. There ensues an interesting shifting of the social scenery. One feels that there is something more involved, and yet fears to invoke Bergson's creative push of life.

ERNEST L. TALBERT

THE UNIVERSITY OF CHICAGO

Social Value. A Study in Economic Theory. (Critical and Constructive.) B. M. ANDERSON, JR. Boston and New York: Houghton Mifflin Company, 1911. Pp. xviii+199.

Dr. Anderson has approached the disputed field of economic value from the point of view of the social psychologist, seeking to identify value as a conscious phase in experience before it becomes the subject of treatment by the economist. His first undertaking is to refute the doctine of both the English and the Austrian schools, which make value a matter of relations either in exchange or in marginal utilities, *i. e.*, in the relation of subjective desires. Against the English school, Dr. Anderson maintains that the ratios of exchange imply a quantity of which the units of exchange are a measure. In the definitions of value by the Austrians the author finds the very idea of value which is to be defined. Even Professor Clark's definition of value as 'social and subjective' proves unsatisfactory because its author merely sums up the individual costs and individual satisfactions connected with economic goods. The economic goods being abstracted from the social organism which gives them their power,

can be estimated only in psychical costs and satisfactions and these being abstracted from the rest of the individual's interests and desires can be estimated only in the economic goods, and again this seeming absolute value revolves in a circle of values each chasing the other.

From this relativity the author appeals to the social organism within which are found the 'funded meanings' which lie back of all values and to the emotional-volitional attitude of the individual which 'motivates' economic action. For Dr. Anderson the theory of value in economics, so far as it is real, is a theory of price, *i. e.*, a measurement of values that themselves cannot be stated in mere numerical ratios. Not even when the Austrian school carries the ratios into the subjective field and finds in the marginal utility of the object an attempted expression of the demand of the individual, does value appear as the relation of desires. "Why has the good, A, value? Because men desire it? No, that is not enough: the men who desire it must have other economic goods, *i. e.*, wealth, with which to buy it. And why will these goods buy it? Because they have *value!*" (46–47). For this school the ratio of desires is not an estimate of the emotional-volitional attitude of the buyer, but of the goods with which he purchases what he desires, which again have value through other desires, etc., etc.

Dr. Anderson makes evident the agreement of much current economic writing with his own position that many social forces beside those economic processes which can be stated in numerical terms, go to change values; that value cannot be spoken of as unchanged while values change. Value as well as values is constantly increased and diminished by legal, ethical and customary changes. But while economic writers are admitting the real influence which social conditions and organization have upon economic goods, their theories and methods hark back to the individualistic period with its contract theory of society and its Humean psychology.

Dr. Anderson would change all this by frankly admitting that these social influences not only modify values, but that values can be conceived to exist only as social phenomena. They arise not through the addition of the pleasures and pains of men, but through organized social activity which is supra-individual. No one individual can express in his wants the value of wheat as it appears in the market and the addition of all men's wants lacks just that social organization by which these wants become articulate to each other and to the men themselves, by which as values they appear in consciousness. The author founds his sociological position on the doctrine

of Professor Cooley's books *Human Nature and the Social Order* and *Social Organization*. Consciousness is social as fundamentally and immediately as it is individual. From this position it follows that conduct is 'motivated' by social forces which cannot be expressed in individualistic states of consciousness, and in this social organization of human conduct, in its enormous implications of meanings which never fully appear in the minds of individuals, can be found the values which get partial measurement in economic terms.

From the sociological statement the author turns to psychology, and after following the structural account of value give by Professor Urban in his *Valuation* leaves this to accept Professor Dewey's functional statement, as more fundamental and more germane to the economic problem.

If social organization gives the funded meanings which back up and enforce values, they lie in individual consciousness as forces, motivations which the author terms emotional-volitional attitudes. Values lead to economic action, they are not relations. Brought into conflict with each other the common quality of value in the objects that calls out the emotional-volitional attitudes is measured. But the measurement is not the quality. Here again not the mere feelings of pleasure and pain, nor the bare desires represent the whole attitude. This involves not only these phases but also a feeling of reality. But what goes beyond all of these elements of structure is the function of the attitude—that of enforcing action, and not merely the act of measurement but the entire conduct of the individual.

Thus in the social and psychological backgrounds of economic measurement and action is placed the value and the values which the economic process partially estimates in the entire conduct of the individual.

The first great service which social philosophy and psychology seems likely to perform for economics, if we follow Dr. Anderson, is to lift from the shoulders of the economists the task of finding and defining value, leaving them only the task of evaluating in terms of price objects and processes in production and distribution. But because the economists will accept value from the hand of the social psychologist and the sociologist, he will be the more ready to recognize the sources out of which the values arise which he is called upon to measure. He will find in his own theory an analysis of existing conditions which abstracts from the full content of experience, and must therefore constantly test its judgments by the changing experience which in its full life it cannot follow.

Dr. Anderson's thesis is most forcibly and clearly written. His acquaintance with the literature of sociology and psychology bearing upon his theme is as full and adequate as it is with that of the economic doctrine of value. It is perhaps too much to ask that he should fill in the gap which still lies between the social sciences and social psychology, but it is worth our while to view this gap afresh in this attempt to use social psychology to interpret economic theory. One of the most striking impressions which the present writer has received from Dr. Anderson's treatise is that his chapters upon the sociology of value and the psychology of value have no functional relationship with each other. Either might have been omitted without seriously affecting the other. Each stands upon its own feet. Following Professor Cooley Dr. Anderson shows that value is a phenomenon of social organization. Following Professor Urban and Dewey he shows that it is an emotional-volitional attitude. What is the functional relation of these two positions? It is impossible to get a definite answer to this in Dr. Anderson's book. While he has affirmed that consciousness is both social and individual, he does not actually place the social organization as the objective world which answers in consciousness to the subjective emotional-volitional attitude. Nor does he ask what are the conditions under which value takes the form of 'funded meaning' in the social organization, nor under what conditions it appears as emotional-volitional attitudes. We are left with the impression that value is a social, supra-individual, reality which can exist in no one consciousness—and yet it does in some sense exist there as emotional-volitional attitude. The final sentence reads: "Ends, aims, purposes, desires, of many men, mutually interacting and mutually determining each other, modifying, stimulating, creating each other, take tangible, determinate shape, as economic values, and the technique of the social economic organization responds and carries them out." This implies that the values arise out of the individual desire through their mutual interactions. But is not the actual situation in consciousness that the social organization that stands over against the self has a different logical character from the 'ends, aims, purposes and desires' of the individual? And is not the process of consciousness, and especially the process of evaluation, a functional interaction between the world as social object and the self as subject. What was funded meaning becomes emotional-volitional attitude, and what is emotional-volitional attitude becomes meaning.

It is in the process of stating the attitude in terms of funded

meaning that we must evaluate it—measure it against other attitudes and determine what is its economic sum as compared with that of other emotional-volitional attitudes that arise demanding their expression. This interaction which Professor Stuart has indicated as the logic of the process of evaluation is left out of Dr. Anderson's treatment of value. That our social psychology is not yet fully adequate to this task is one of the important conclusions that may be drawn from this brilliant volume.

G. H. M.

RELIGIOUS PSYCHOLOGY

An Empirical Study of Prayer. JAMES BISSETT PRATT. *Amer. J. of Relig. Psychol. and Educ.*, 1910, 4, 47–67.

The questionnaire method is used. "In what do people's prayers actually consist? What is their nature and content? How are they used? and How well do they serve their purpose?" Prayer is actually practised for the purpose of petition or communion. Half of those whose prayers are largely petitions do not believe that God is changed by them; they continue the practise of such prayers because of the impulse to pray and because of the subjective benefits experienced from the actual formulation of these petitions. Other answers note the exertion of an influence (1) upon the mind of him who prays, (2) upon the mind of others, (3) upon material things—relief from personal danger, victory in athletics, and the cure of sickness. All these may be explained psychologically in terms of suggestion or autosuggestion. The experience known as the sense of communion with God is an interesting part of the psychology of religion. The invigorating influence of prayer is an unquestionable fact of empirical psychology. Prayer is one of the living forces like faith in harvest and the love of children.

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Is Faith a Form of Feeling? A. C. ARMSTRONG. *The Harvard Theol. Rev.*, 1911, 4, 71–79.

Faith in the form of feeling was commended in eras of doubt as "a substitute for the halting processes of reason with their dubious or negative conclusions." This motive has markedly influenced the religious development of modern times. Man's emotional life is essentially connected with his cognition; its bodily side apart, it is

feeling related to idea. Hence the resolution of faith into mere feeling overlooks an essential element in the total phenomenon, namely, the intellectual aspect of religion.

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THE UNIVERSITY OF CHICAGO

Aufgabe und Methode der Religionspsychologie. HERMANN BANKE. *Zsch. f. Religionspsychologie*, 1911, 5, 97–104.

The author's chief criticisms of the American presentation of religious psychology, made on the basis of the works of Professor James and Professor Starbuck, concern the following questions: (1) The natural scientific method used, (2) the relation of history and psychology to the formation of religious values and judgments, (3) that which Wobbermin calls the consideration of the question of truth, which is really the relation of psychology to the normative theory of knowledge in the psychology of religion. According to the author's view, psychology sets forth a theory of knowledge; this is only possible with the help of functions, psychologically determined. But this should form the introduction to a formal study of the science of religion; for an empirical psychological study, only a formal concept of religion is necessary. As soon as one sets forth a definite conclusion as to religious values, he leaves the field of psychology of religion for that of the philosophy of religion.

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Religion als Seelenkunst. WILHELM GEORGE. *Zsch. f. Religionspsychologie*, 1910, 4, 196–208.

Law and customs, religion and art, are different radiations from national and racial character. In law and customs the objective social law comes to realization; in art and religion the individual's subjective demands and needs are uppermost. Religion may be called an art of the 'soul,' a creative fashioning produced in the world of ideas under deep feeling and undertaken on the basis of faith in the value of action.

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The Ethnological Background of the Eucharist. J. A. MAGNI. *Amer. J. of Relig. Psychol. and Educ.*, 1910, 4, 1–47.

To discover the root idea of sacrament is the object of this article. The idea is found in the fact that whatever object was believed to

be the embodiment of the deity in primitive times was sacramentally eaten for the purpose of absorbing the divine attributes and for nursing and strengthening the physical bond between the tribe and its totem god. The genetic development is shown in the ethnological background of the Orient and particularly of Asia Minor from 600 B.C. to the early Christian era, and in the striking parallels of the Mithraic and Gnostic cults in which the primitive savage belief had been revived and disseminated in the form of mystic and sacramental communion. The age of Christianity in Asia Minor is a synthesis of Semitic, Greek, and possibly Indian thought. The Christian sacrament is Pauline in origin and Paul a child of a credulous, uncritical age, his mind absorbing unconsciously the prevalent idea. The Eucharist is of ancient pagan origin, having become a part of the Christian cult by a process of theological speculation on the meaning of Christ's death, resurrection, and mission.

C. M. Breyfogle

The University of Chicago

NOTES AND NEWS

The present number of the Bulletin, dealing especially with social and religious psychology, has been prepared under the editorial care of Professor G. H. Mead.

The following items are taken from the press:

Professor Joseph Jastrow, of the University of Wisconsin, gave a public lecture entitled "On the Trail of the Subconscious," at the university on December 4, under the auspices of the university association for research and Phi Beta Kappa.

With the issue for January–March, 1912, *The American Journal of Religious Psychology and Education* will become the *Journal of Religious Psychology, including its Anthropological and Sociological Aspects*, having as editors President G. Stanley Hall and Professor Alexander F. Chamberlain. The journal will continue to be published at Clark University under the auspices of the library.

Publisher's Note.—Owing to the great demand for space, the Psychological Review and Psychological Bulletin will be increased to about 480 pages each next year. From January 1, 1912, the subscription to the Review and Bulletin will be $5. During the present month subscriptions may be renewed at the old rate ($4.50).

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Names of contributors are printed in SMALL CAPITALS, and the page numbers of the contributions in **Full Face Type.** In the case of authors reviewed or summarized the page numbers are in *Italics* and in the case of mention in the notes they are in Roman Type.

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